JOURNAL

OF THE

INSTITUTION OF ELECTRICAL ENGINEERS,

INCLUDING

RIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

BLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE

AND EDITED BY

W. G. McMILLAN, SECRETARY.

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The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers.

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LATE

THE SOCIETY OF TELEGRAPH-ENGINEERS AND ELECTRICIANS.

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The minutes of the Annual General Meeting held on May 22, 1902, were read and confirmed.

The CHAIRMAN: Gentlemen, I have an announcement to make which I am sure will be received with a universal expression of regret. We all met here to-night in the hopes of hearing a most attractive and instructive address from our President, but the Council has been advised that he is too ill to appear. The question as to whether the reading of his address should be dealt with by deputy to-night, or whether it should be postponed until the President himself could be present, has been fully considered. Mr. Swinburne, in order to prevent any feeling of disappointment, was rather anxious that it should be read by deputy, but the Council, after giving the question the most earnest consideration, came to the conclusion that the reading of the Presidential address in the President's absence would be something like the play of *Hamlet* with Hamlet left out, and it determined to postpone the reading of the address to some date to be fixed hereafter. I am sure you will all unite with me in expressing the feeling of regret at the attack of illness from which our President is suffering, an attack which I venture to hope is not a dangerous one, although sufficiently serious to incapacitate him from being present this evening. I ask you to authorise the Secretary to advise Mr. Swinburne that the members here present unite with the Council in an expression of regret at his unavoidable absence.

The names of new candidates for election into the Institution were announced, and it was ordered that their names should be suspended in the Library.

The following transfers were announced as having been approved by the Council:—

From the class of Associate Members to that of Members—

G. W. Green.	A. H. Shaw.

From the class of Associates to that of Members—

Capt. H. B. Jackson.	J. W. Leyshon.

F. S. Pilling.

From the class of Associates to that of Associate Members—

W. F. Bruce.	James Nicolson.
J. E. Dawson.	J. G. Scott.
G. L. Eynon.	E. M. Sellon.
Ll. L. Foster.	Percy Speedy.
R. W. Hammond.	James Whitcher.

From the class of Students to that of Associates—

R. D. T. Alexander.	J. W. Griggs.
J. Bentley.	R. P. Howgrave-Graham.
H. E. Britton.	G. W. Mayne.
E. Brown.	C. H. Millar.
J. F. Caine.	T. Normoyle.
H. H. Clements.	H. A. Pearson.
M. A. Codd.	J. St Vincent Pletts.
A. M. Coombs.	H. K. Rodwell.
W. A. Del Mar.	T. G. Smith.
W. H. Derriman.	A. Sommerville.
P. A. Fisher.	L. Vignoles.
P. Good.	A. R. Walmsley.

E. V. Watson.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. Alby, The Astronomer Royal, Cassell & Co., "Colliery Guardian," H. Cuénod, Editor of "Electricity," Institute of Mining Engineers, International Engineering Congress, Glasgow, 1901; E. Jona, Maschinenfabrik Oerlikon, Meteorological Society, B. H. Morgan, Patent Office, R. E. Peake, Royal Society, Rev. G. H. Staite, Teknisk Forenings Tidskrift, W. P. Thompson, Prof. Wyssling, and from Dr. H. Wilde, F.R.S., Honorary Member; Messrs. C. Bright, E. Danvers, L. W. de Grave, J. J. Fahie, B. T. Ffinch, R. K. Gray, Dr. Alfred Hay, W. P. Maycock, J. W. Meares, G. D. A. Parr, Members; and L. Birks, Associate Member; to the *Building Fund* from Messrs. H. O. F. Bindemann, B. G. Burgess, A. J. Cridge, W. P. Digby, H. W. W. Dix, L. Drugman, S. Evershed, The Finsbury Technical College Engineering Society, G. Johnson, J. Kynoch, C. H. McCarthy-Jones, J. C. Matthews, C. F. Proctor, H. M. Sayers, E. S. Shoults, H. D. Symons, F. H. Webb; and to the *Benevolent Fund* from Mr. F. H. Medhurst, to *whom the thanks of the* meeting were duly accorded.

The CHAIRMAN: I have very great regret in announcing that several deaths have occurred since our last Session. Amongst them are those of Sir Frederick Abel, who was elected a member in 1871, and President in 1877, and who has been a trustee since 1887; Dr. John Hall Gladstone, elected a member in 1873, and a member of Council in 1887; and Professor Sidney H. Short, elected a member in 1901, and a member of the Sectional Committee on Traction, Light, and Power Distribution during 1901 and 1902. I have no doubt that the members will sympathise very heartily with the friends of these gentlemen, most of whom have for so long been associated with us.

APPOINTMENT OF NEW HON. TREASURER.

I have another announcement to make which I feel sure will be received with considerable regret by all our members. Our Honorary Treasurer, Professor W. E. Ayrton, who has held office for so long a time in various capacities, has felt himself compelled, partly owing to ill-health and partly to the pressure of other duties, to resign his office of Honorary Treasurer. We all know our dear and respected friend Professor Ayrton. He was one of our very early members, and we have followed his career with interest and best wishes. We know what erudition he has brought to bear on our debates, and what a useful member he has been to our Institution. He has also devoted a vast amount of time and attention to the interests of the Institution, both at the Council and Committee Meetings, and as Honorary Treasurer. Many of you who have not served in these offices will, in a later period of your lives, when called upon to help to direct the destinies of an Institution of this sort, perhaps appreciate more fully than may now be the case, the amount of work and the sacrifice of valuable time that is necessary in order to carry to a successful issue the affairs of our Institution. I feel sure that you will all unite in a very hearty vote of thanks to Professor Ayrton for the lengthy and honourable service that he has rendered to the Institution.

The vote was carried by acclamation.

The CHAIRMAN: Gentlemen, a very old adage, *Le roi est mort, vive le roi*, applies in this instance as in most others. I am happy to announce that Mr. Robert Hammond has been elected, and has undertaken to fill the onerous office of Honorary Treasurer in succession to Professor Ayrton.

VISITS TO ITALY AND AMERICA.

The Council has had under consideration the continuation of those useful visits to foreign works which were inaugurated a few years ago, and which have proved to be so successful and such a source, not only of pleasure, but, may I venture to say, of educational value to many of our members. It has had under consideration the arrangement of a visit to Italy in the course of next year, and I am now in a position to announce the preliminary arrangements. It is proposed to travel viâ Lucerne and *the St. Gothard* route, leaving England on Thursday,

April 2, 1903, and arriving at Como on Friday, April 3rd ; the party will leave Como for Milan on Monday, April 6th, and will disperse at Milan on Thursday, April 9th, the day before Good Friday, so that the members may, if they wish, spend the Easter holidays in Italy, or in Switzerland, *en route* home. I need not say that this will prove a very interesting visit. Our co-engineers in Northern Italy have availed themselves to the utmost of the great water powers that are stored up in the Alps, and we may anticipate not only a most instructive but, from the picturesque point of view, a most pleasant and interesting journey.

I have further to announce that the Institution has received an invitation from the American Institute of Electrical Engineers to visit the United States, and to hold a joint meeting there or in Canada. I have been asked to read this letter on account of its warmth of tone and the welcome that it offers to the members of this Institution :—

"AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.
NEW YORK, *July* 21, 1902.

"INSTITUTION OF ELECTRICAL ENGINEERS,
Victoria Mansions,
28, Victoria Street,
London, S.W.

"GENTLEMEN,—There has been a growing desire among the members of the American Institute of Electrical Engineers that arrangements should be made for an official visit of its sister society, the Institution of Electrical Engineers, to this country in the near future. This desire has been fostered and stimulated by the pleasant recollections of those of our members who attended the joint meeting of the two Societies at Paris in 1900, more especially by those who had the good fortune to participate in the enjoyable programme of entertainments and social courtesies extended to the American Institute of Electrical Engineers by the Institution and its friends in England, prior to the Paris meeting.

"This desire has just found formal expression before our Institute ; and I have great pleasure in officially informing you that at the recent Annual Convention held at Great Barrington, Mass, June 18 to 21, 1902, it was decided by unanimous resolution that the American Institute of Electrical Engineers do invite the Institution of Electrical Engineers of Great Britain to take part in a joint meeting in this country. The time and place were left undecided, the details of arrangement being referred to, and placed in the hands of, the Committee on Meetings. There were several reasons why it was deemed inexpedient to fix definitely the date and place in the resolution aforesaid, which reasons I will proceed to explain.

"Suggestions were made and considered at the Convention contemplating a joint meeting, either during the year 1903 or the year 1904. The Committee on Meetings is desirous that the Institution of Electrical Engineers, as the intended guest of the American Institute of Electrical Engineers, should first be consulted and should be allowed

to express its preference in regard to both the time and the place of meeting, before fixing the same, in order to avoid deranging any plans which the Institution might have made definitely or might have in view for the year 1904. For this reason, instead of submitting a definite programme and tendering a definite invitation to the Institution, it is deemed preferable to submit to the Institution the outline of two alternative plans.

"The first plan contemplates a meeting of the American Institute of Electrical Engineers at Montreal in the year 1903. At the recent Annual Convention just referred to, a very cordial invitation was extended to the Institute by the McGill University at Montreal, to hold its Convention of 1903 in the Canadian metropolis. It was recognised and universally admitted that such a convention would be more important and enjoyable if the Institution could also arrange to meet with us on that occasion. In such case there would doubtless be a programme of reception and entertainment in New York, either prior or subsequent to the meeting in Montreal.

"The second plan would be to defer the joint meeting until the year 1904, that being the year during which the Louisiana Purchase Exposition is to be held at St. Louis, Mo. This Exposition promises to be a very important affair, since over thirty million dollars will be expended in carrying out the elaborate and comprehensive plans of the Exposition Committee. It is proposed to hold various Congresses, including an International Electrical Congress, at St. Louis during the Exposition. In view of these facts, the Committee think that many members might prefer to have the official visit of the Institution deferred until the year 1904. In such case, the joint Convention would be held in the eastern section of this country, where the American Institute would be pleased to entertain its foreign guests. Our members could subsequently proceed with them to the Exposition and Congress at St. Louis.

"It is, therefore, my pleasure to submit the Institution of Electrical Engineers, on behalf of the American Institute of Electrical Engineers, a hearty invitation to meet with us during the season of 1903 to 1904, as may seem to the Council of the Institution the more convenient and desirable.

"As already stated, it is the desire and purpose of the Institute to conform to the future plans of the Institution, in order that the largest possible number may participate in the Convention. The Committee on Meetings will begin the work of preparing a programme as soon as we receive from you an intimation of your preference and desires in the matter.

"Trusting that we may unite in perfecting a plan that will redound to the interests of both Societies, I have the honour to remain,

"Yours most respectfully,

(Signed) CHARLES P. STEINMETZ, President."

On account of the Italian visit, the American visit could not be arranged for 1903; hence it is accepted for 1904.

General Announcements.

I have further to announce that a deputation of the Council has recently waited on the President of the Board of Trade in connection with the proposed revision of the Board of Trade Regulations. The matter is not ripe now for definite announcement of resolutions, but it is hoped that full particulars may be furnished to the members at a future date.

The Council has been in correspondence with the Home Office in regard to the Factory Act, and has been assured that a full opportunity will be afforded for the discussion of the new Regulations before they are adopted. The Council further have reason to hope that the Secretary of State may see his way to take such steps as he can under the present Act of Parliament to assist the profession in regard to the employment of young persons in electricity works.

I have also another satisfactory announcement to make, namely, that the President of the Institution of Electrical Engineers has been appointed *ex officio* to serve on the Committee nominated by the Home Office to inquire into questions relating to the use of electricity in mines.

On the subject of professional etiquette the Council has issued a code for the convenience of members, and has published it in the technical press. I presume that all the members have made themselves acquainted with the details, and that it is unnecessary for me to dwell further on the subject.

Subscriptions and *Science Abstracts*.

Now I approach what may be possibly considered a very burning question, that relating to subscription rates and the issue of *Science Abstracts*. On June 9th, when the circular in relation to the proposed alteration of the rates of subscription was issued, the Council felt that it was imperatively necessary to increase the amount of the funds at its disposal. The question, I need not tell you, has received very earnest consideration, and the letter referred to was issued in order to elicit the opinion of the members generally. A considerable number of the members have replied, and have expressed their views. I think I may say that, generally speaking, they are prepared to support the Council in the measures that it considers necessary to adopt for the benefit of the Institution. As the result of the fullest consideration, not only of the question itself but of the views expressed by the members, the Council is now prepared to recommend rates which I propose to read to you. It considers that at the present time the rates must be levelled up to the ordinary maximum of each class. Further, it has considered that it is reasonable and desirable to differentiate between members resident abroad and those at home, who get all the privileges of membership and who are able to attend the meetings in London or the sectional meetings that have been established in the Provinces. The recommendations, therefore, are that the whole of the Members in this country shall pay a uniform rate of three guineas, which is the ordinary rate at which new Members enter, and that Members residing abroad

shall pay two guineas; Associate Members at home shall pay two guineas, and abroad one and a half guineas; Associates at home two guineas, and abroad one and a half guineas.

Now I come to another question dealing with the students. Under the present regulations, a student after remaining in the class of students for three years is compelled to become either an Associate or an Associate Member, or to leave the Institution; but it is proposed now to divide the students into two classes, namely, students junior and students senior. For the first three years of a student's connection with the Institution he will pay a guinea annually, or if he be then under 22 years of age, until he attains that age; but he may then, if he wish, and if under 26 years of age, remain until he is 26 as a senior student at the annual payment of one and a half guineas. A meeting of the full members of the Institution will shortly be called, and these proposals will be laid before them for consideration.

I now come to the question of *Science Abstracts*. This question has engaged the attention of the Council very seriously for a long time. *Science Abstracts*, I need not say, is a most valuable publication to all interested in the progress of our science. But the cost of its production has increased and is increasing, and from time to time the question of what amount the Council could afford to pay as its share of this cost has had to be debated. Now it has been felt that the free and gratuitous distribution of *Science Abstracts* to all and sundry of the members, whether they value the publication or not, was perhaps a mistake. Instead, therefore, of adhering to the practice of the free issue of *Science Abstracts* to all classes of members, the Council concluded that if the rates that I have announced were combined with a moderate subscription from those who required the issue of *Science Abstracts*, the equity of the case would be best met as applied to the whole of the members generally. The proposal, therefore, is that those members who wish to be furnished with copies of *Science Abstracts* shall have their wishes met on the payment of an annual subscription for both parts, that is, physical and engineering, of 7s. 6d. per annum, or the engineering portion alone for 5s. I think that deals broadly with the general question of funds. I may say in relation to these proposals that the ordinary publication price of *Science Abstracts* is 24s. per annum.

Approaching Award of Willans Premium.

I am further asked to announce in reference to the Willans Premium to be awarded by this Institution in December, 1903, the Council will, under the Trust by which it is bound, award the Willans Premium "to the best original paper contributed to the Institution dealing with such a general subject as the utilisation or transformation of energy treated specially from the point of view of efficiency and economy, and that the premium shall not be awarded unless a paper of sufficient merit, in the judgment of the Awarding Council, shall have been so communicated since the preceding award of that Council."

The premiums reported in the Annual Report presented in May, 1902, were then presented.

The Three Hundred and Eighty-second Ordinary General Meeting[1] of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, December 4th, 1902—Mr. J. SWINBURNE, President, in the Chair.

The minutes of the Ordinary General Meeting held on November 22, 1902, were read and confirmed.

The names of the candidates for election into the Institution were announced, and it was ordered that these names should be suspended in the Library.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

Reginald Robert Todd.

From the class of Associates to that of Associate Members—

Gerald Carlyle Allingham.
H. Borns.
Frank Cobden Briggs.
Denis Ripley Broadbent.
Frederick W. Close.
Percy Rhodes Cobb.
Walter Eynon.
Robert Loraine Gamlen.
Harry Philip Gaze.
Harry Percy Girling.
Alexander Glegg.
Selwyn Seafield Grant.
Reginald Charles Harpur.
John Henderson.
Alfred Edward Jackson.
Louis Campbell Login.
Owen David Lucas.
Rowland Marshall.
Francis Miller.
Walter Victor Morten.
Arthur Holroyd Sears.
Charles Arthur Slater.
David Smith.
David Brown Walker.
Charles Aspull Wells.
C. Barnard Wigg.
Reginald Page Wilson.

From the class of Students to that of Associates—

William Hunton.

Messrs. H. G. Wood and P. G. Timms were appointed scrutineers of the ballot for the election of new members.

A donation to the *Library* was announced as having been received since the last meeting from Dr. H. Borns, to whom the thanks of the meeting were duly accorded.

[1] This was an Extra Meeting called to enable members to hear the President's Address, postponed from the Meeting of November 13th. It is *therefore printed out of* order, and before the Proceedings of November 27th.

The PRESIDENT: Before delivering my formal address I would like first of all to thank the Council, and what is the same thing, to thank the Institution, for the kind way they treated me a fortnight ago when I was unable to read my address.

I also desire to refer to the work of my predecessor, Mr. Langdon. We are all very sorry that Mr. Langdon is not here to-night. I do not think it could do any harm for this Institution to realise if possible a little more the work that Mr. Langdon did for us. Mr. Langdon was handicapped very severely, to begin with, by living up in the Midlands. In spite of that he attended—I forget how many—but something like 82 or 92 Council and Committee Meetings. If any one of you who is living a busy professional life in London realises what it would be to attend 80 or 90 meetings in Birmingham, he will understand the sort of self-denying work that Mr. Langdon did for this Institution. In addition to that, Mr. Langdon's health was not very good, and I think he sacrificed himself on behalf of the Institution several times. Also I wish to refer to a rather marked change of policy in the Institution during Mr. Langdon's Presidency. We owe in a great respect to Mr. Langdon the change in the attitude of the Institution towards making it more commercial, and keeping it more in touch with the purely technical part of our large interests. Mr. Langdon took a very keen interest in all the work of the Institution, and I may mention particularly the questions which ended in a deputation to the President of the Board of Trade. You remember that I was spokesman on that occasion, but as a matter of fact I had only just come into office, and the whole of the work was really done by Mr. Langdon—at least the whole of the Presidential work—and we owe a great deal to him for any good that may have come indirectly—and I believe a great deal of good has come—from the action of the Institution in that matter.

SOME LIMITS IN HEAVY ELECTRICAL ENGINEERING.

INAUGURAL ADDRESS BY THE PRESIDENT,

JAMES SWINBURNE.

Argument.

Two kinds of limit.—Electrical means scientific engineering.—Engineering is Science.—Unapplied and applied science complementary.—Engineering includes both raw and finished science.—Raw science important.—Unfortunate attitude of raw towards finished science.—Development of tidal power not practical.—Fictitious value attributed to water-powers.—Electrical energy direct from coal a dream.—Limits of efficiency of steam engine.—Steam and SO_2.—(Notes. Units. Entropy.)—Limits of gas engine.—Large margin for improvement.—Dynamos and transformers near limit.—Secondary battery progress limited chemically.—Little chance of great improvement.—Cables made empirically; room for advance in insulation, but not in con-

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Tides.

The tides are often referred to as a possible source of energy even to this day; and it is urged that in places where the tide rises abnormally, for instance in the estuary of the Severn, it would pay to make a dam with turbines. The sort of argument is that if you have an area of, say, 1,000 square metres, and a total rise of 15 metres, you have 15,000 cubic metres of water, and as this runs in twice and out twice a day, you have 15,000 cubic metres of water, falling the equivalent of 60 metres a day; or approximately 100 kilowatts. This statement contains many fallacies. In the first place, in order to get the full advantage of the difference of level the water must be let in and out at high and low tide only. Even then the equivalent, or average head during discharge or charge, is only 7½ metres. But a system which gave an enormous power for a very short time four times a day would be of no use. The plant would be expensive, and the result of no value. With a single tank it is impossible to get a continuous output. If the tide is coming in, and you get power by letting the tide fill the tank, the power will decrease to zero as the tide begins to fall and comes to the same level as the water in the tank. It is, therefore, necessary to have more than one tank. To make the plant practical you want fairly constant pressure available on the turbines, though you may waste head by sluices or valves. Then the tank may be divided up into three, one of 200, and two of 400 square metres. The small one is emptied at each low tide and filled at each high tide. Starting at half tide, with the tide rising, and the 200 and one 400 metre vat empty, and the other 400 quite full, the small tank takes in water at such a rate that at high tide it is half full. It thus takes in 1,500 cubic metres of water with a useful head of 7½ metres for three hours. This gives 10 kilowatts. At full tide the large empty tank is allowed to fill through a turbine working on 3¾ metres, but taking twice as much water per minute, and this goes on till half ebb tide. The small tank is rapidly filled to high tide level. At half tide the large tank has 3¾ metres of water, and the head is getting too small, so the small tank is now allowed to empty for three hours with 7½ metres fall, until low tide. From low tide to half tide the second large tank is emptying from the 15 to the 11¼ metre level. By means of these tanks, of total area of 1,000 metres, and two turbines and one dynamo we thus get 10 kilowatts going into the turbines. If the tide—and the neap tide must be taken—is only, say, 4 metres, we only get 700 watts! No doubt the tanks and turbines might be worked somewhat more profitably than I have sketched, but there can be little margin. Turbines to work on variable pressures, or any sort of storage, mean more capital expenditure, and it is the great capital expenditure that wrecks tide schemes. It is often said that a Norwegian fiord or a Scotch loch could be easily dammed and utilised; but it would be impossible to find three lochs all opening out together. The need for more than one reservoir does not seem to have been recognised. In addition the demand for electrical energy on Scotch lochs or Norwegian fiords is rather *minute.*

Water Power.

Some years ago there was a great deal of excitement about the development of water powers. The possibility of "harnessing Niagara" and utilising water-falls all over the world was hailed as a great triumph over Nature, and the idea was that power could be got for nothing, and industries would all migrate from coal districts to the neighbourhood of water powers. The daily press and the magazines took the matter up, and there is something in the idea of saving some of the colossal waste of natural energy that appealed especially to the half-scientific or unpractical reader. At the time of the excitement it was pointed out, largely in vain, that water power did not cost nothing, because the development of a fall demanded a good deal of capital, whose interest and depreciation had to be paid. But further than this, Ricardo's theory of rent is applicable to water powers as well as to arable land. If steam power costs a farthing a unit, and if water power at the same place can be produced for half a farthing, after paying working expenses and interest, the owner of the water power will claim the odd half farthing as rent, or will just allow the water power enough to encourage the production of a new thing. As a rule, however, a water power is not where it is wanted industrially. In the nature of things water powers are generally in hilly countries, and are seldom near the sea. The result is that a water-power as a rule cannot command the same price as steam or gas, because it is not where it is wanted. The idea in starting many of the water-power stations also was that works which needed power would come and settle near. As a matter of fact the cost of power is a much smaller item in most industries than is generally supposed, and it does not pay to start a works in an otherwise not perfectly suitable locality, simply for the sake of the cheap water power. In such industries as engine building, flour milling, spinning and weaving, and so on, the chance of reducing the expense for power is not enough to overcome other considerations. It may said that in electro-metallurgical processes the whole cost is practically the electrical energy, and so carbides, aluminium, and electrolytic soda, and chlorate of potash will be made at water powers. Even this, however, is misleading. Carbides and aluminium are generally made at water-falls, and chlorate nearly always is. Electrolytic soda and bleach are made at water powers, but are also made extensively by steam-driven plant. Against the cheaper power, we have to put extra carriage for materials and for coal, which is often needed in addition, and extra carriage for finished products, and very often extra cost of labour, as labour is often dear and bad in water-power districts. Let us take, as an example, calcium carbide. The general idea is that the electrical energy is practically the whole cost of the carbide. Taking present practice, however, a kilowatt makes about two tonnes, or say two tons of carbide a year. The difference between water at, say, £2 10s. and £5 a kilowatt year is thus £1 5s. a ton in cost. The price of carbide may be taken at £13 10s. a ton, so doubling the cost of power instead of nearly doubling the price of the carbide would increase *it a little more than* 10 per cent. Difference in local cost of

coke, lime, and labour, coupled with cost of carriage, may thus easily be of more importance than cheap power even in such a case as calcium carbide, which is an electrical furnace product in which, at first sight, the power seems to be the main element of cost. In the case of electrolytic caustic and bleach, for one ton of caustic and the corresponding bleaching powder, the electrical energy, taken at £2 10s. per kilowatt year, a low water-power cost, comes to about 17s. 6d. The caustic and bleach sell for about £20, according to a varying market. Doubling the price of power therefore increases the price some 5 per cent. It may thus easily pay to use much more expensive power, if the other conditions are more favourable. Steam power, for instance, will cost 3 or 3½ times as much, and yet it pays to make electrolytic caustic and bleach in England where the other conditions are all favourable. It is not, therefore, the want of water power that has kept the electrolytic industry back in this country. For a water power to be really valuable, it should be near a source of material, on the sea, and should have a great head of water, so that the capital cost of development is small. Such a water power is very valuable—to the landlord.

A blast furnace is more valuable than a water power. There are plenty in England. But the owners, who have been wasting the gas up to now will not give it away; they will want rent, so that it will only just pay to use his gas rather than make it. The electrical industry thus does not gain, but the iron-masters do.

Carbon Cells.

For many years "electrical energy direct from coal" has been the dream of the electro-chemist. That is to say, he has dreamed of an electrolytic cell in which the consumed electrode is carbon. The best way to realise the difficulties of this problem is to consider it solved and see what it means. The carbon must be in contact with an electrolyte, and that electrolyte must either be in contact with a second electrolyte which wets the other electrode, or must itself be in contact with that electrode. This second electrode must almost certainly be metal, as there are no other non-metallic conductors available. The electrolyte in contact with the carbon must be a salt of carbon, or must contain a salt of carbon, or it must contain another salt whose positive radicle can be replaced by carbon. Such compounds as the hydrides, nitride, oxides, chloride, bromide, or the sulphide, or silicide, of carbon are not salts in the electrolytic sense. Carbon forms part of the electro-positive radicle in the organic radicles, and part of the electro-negative radicle in the cyanogen compounds, but it is never a radicle by itself. To sum up the matter shortly in the light of modern theory, carbon never forms ions, and has therefore no solution pressure, and can therefore give no electromotive force.[1] At ordinary or moderate temperatures carbon is practically inert. Oxidising agents will attack some forms slightly; and sulphuric acid will attack it. In this latter case the formation of water and its combination with the acid

[1] *See* however, Billitzer, Monatsh. 1902, 23, 502.

is the determining factor. At high temperatures, oxygen, sulphur, silicon, and to some extent nitrogen, and many of the metals, combine with carbon, but there is no dissociable salt of carbon formed. The carbon cell thus seems impossible. Such schemes as Mr. Reed's, ingenious as it is, is not a solution of the problem. It would be simpler to reduce zinc oxide with the carbon, and then put it in a zinc cell.[1]

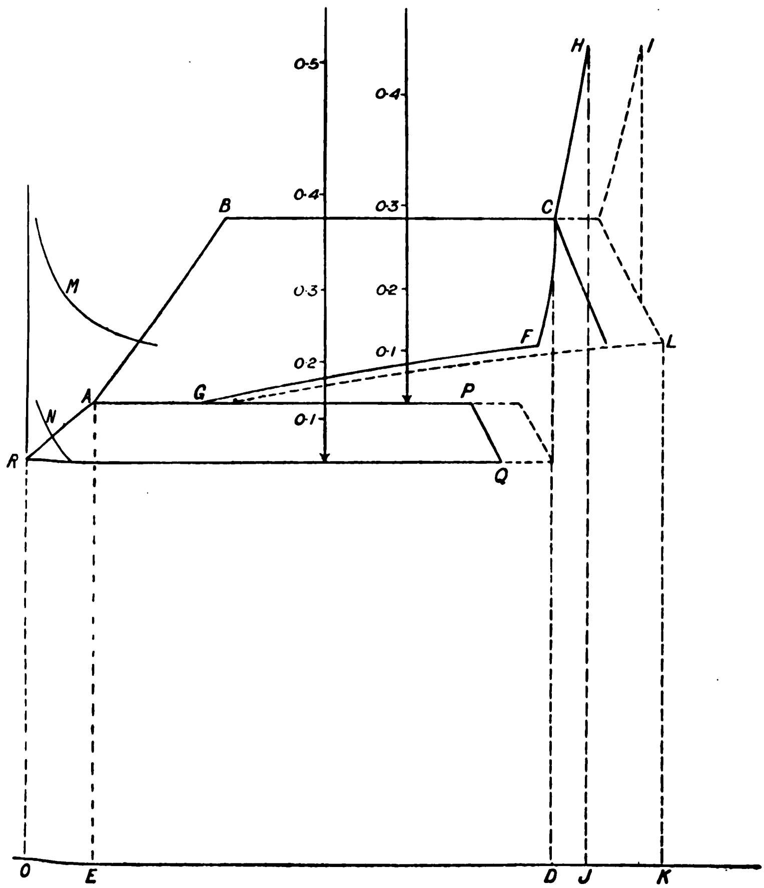


FIG. 1.—θ,χ and θ,ϕ curves for Steam and Sulphur Dioxide Engine.

It is hardly necessary to discuss thermopiles or thermo-magnetic engines as possible economical producers of electric power.

Steam Engines.

The primary question in all heat motors is: What temperature

[1] For a full discussion of the carbon cell see *Primary Batteries*, by W. R. Cooper. E. de Fodor, in *Elektricität direkt aus Kohle*, Hartleben, Leipzig, also gives a full account of work done on this problem up to 1897, the date of the book.

range is available? In the case of a steam engine there is enormous waste of mutivity [1]—to use a variation of Lord Kelvin's convenient term—in boiler flues. We burn carbon and hydrogen, capable even with air of giving a temperature of some 1,500° C., and the heat is degraded down to some 200° C. That is to say, instead of getting the heat with a mutivity of about 0·825, we degrade it down to, say, 0·35, a clear loss of 0·45 out of 0·8, or 56 per cent. This degradation is apart from the efficiency; the efficiency is concerned with the loss of heat up the chimney. The higher limit in large modern reciprocating engines may be taken roughly at 600° A. (327° C. or 620° F.). Above this there is difficulty in lubrication, and to some extent weakening of the material. The pressure corresponding to this temperature for saturated steam is out of the question, and the pressure may be taken at, say, 12·5 megadynes per square centimetre or 12½ atmospheres, [2] or 200 lbs. per square inch, and steam leaving the boiler superheated to 600° A. does not get at the cylinder lubrication at that temperature. Our limits in the steam engine are thus pretty clearly defined. The pressure is the essential factor. Superheating is not much good in the way of getting higher mutivity in the boiler, nor is it very important in getting much more energy into the steam. This is shown diagrammatically in Fig. 1. The vertical ordinates are absolute temperature, but in Centigrade degrees, and the horizontal distance represents the quantity factor corresponding with temperature, so that the area is the energy of a gramme of steam. That is to say, the energy in the gramme of steam at any point in the cycle is the part of the whole area included in a horizontal line from that point to the left, and a vertical line to the zero line at the bottom of the diagram. The figure A B C D E is thus the energy taken by the steam, of which A B C F G is used and the rest wasted. The mutivity scale for a lower temperature of 338° A. shows the small value of the energy taken in. This curve is for temperature 473° A. and 338° A., 200° C. and 68° C., and the exhaust is opened at 382° A. causing the wedge-shaped loss on the lower right-hand corner. The expansion is taken as adiabatic without cylinder cooling. To the right is shown dotted to the same scale the ordinary temperature entropy diagram. [3]

The mutivity scale shows that superheating up to H on the energy or I on the entropy curve, gives little advantage from the mutivity point of view, while the extra energy C H J D is small. Superheated steam thus carries but little extra energy, and carries that little without much extra mutivity. But superheating steam is very important for ordinary engines as it reduces the expansion condensation, and what is much more important, the cylinder condensation. It has been proposed to reduce cylinder condensation by coating the piston and cylinder cover. Perhaps enamelling them would help. The walls even in a short fat cylinder give most trouble. Hadfield has proposed alloying the surface. The alloy would have to be mechanically suitable and ought to have low specific heat and low conductivity.

[1] See Note A, p. 36. "Mutivity."
[2] See Note B, p. 37, "Common Sense and Scientific Units."
[3] See Note C, p. 37, "Entropy."

Jacketing is also used to reduce condensation. As long as the cylinder is hotter than the steam there can be little communication of heat; the trouble is, that if the surface is wet evaporation cools the metal. The jacket should therefore keep up such a flow of heat through the cylinder walls that there is no deposit from expansion condensation, so that the walls never get wet. Pouring heat through a cylinder wall into colder steam is itself an uneconomical process, meaning increase of entropy. Superheating proves to be the easiest and most economical way of dealing with a source of waste that is inherent in a reciprocating engine. But the weight of steam should not alone be used in comparing the performance of an engine with superheated steam with another. Throughout the pressures in common use a kilogram of saturated steam takes sensibly the same heat to evaporate it, and there is such a great fall of mutivity in the flues that a boiler can evaporate at one temperature nearly as economically as at another, so the weight of steam used is a measure of the badness of the engine. But superheated steam takes more heat to produce it, so that a boiler with a given coal consumption and chimney temperature gives less.

Our upper limit in the engine is thus somewhere about 473° A. for saturated steam with a mutivity of, say, 0·285, with a little addition up to 600° A., with an average mutivity of, say, 0·37. An extra temperature or "superheat," as it is somewhat barbarously called, of 127° thus would give very little advantage if it were not for the inherent badness of the engine. I am here dealing with large high-class engines. Superheating is of very much greater advantage with the ordinary small and middle-sized engine in common use. The lower limit is, however, of great importance. If we could work down to the temperature of the air we would gain greatly. We cannot do so because the engine would have to be enormous. The θ,v curve is shown at M to a small scale. It shows the volume becoming unmanageable. A great deal of condenser water would also be needed. A two-fluid engine is theoretically possible. We want a second fluid which gives a higher pressure at, say, the temperature of a condenser, or better at the temperature at which it is inconvenient to expand steam any further. I have taken sulphur dioxide as an example, because most of the data are available, and because it is being tried in Germany, though I have not seen any accounts of results. If a higher temperature than the one I have taken were used, the lower right-hand corner of the steam diagram would be saved. The lower closed area A P Q R is the energy curve of the corresponding weight of SO_2. I have only considered the internal energy, or U, of the fluids for simplicity. It is accurate enough for the purpose of explanation. The curves are not minutely accurate either. The additional area A P Q R is thus, theoretically, clear saving due to the use of SO_2, which enables a second engine of reasonable size, as it were, to continue the useful expansion down to a temperature little above that of the air. The θ,v curve N, which takes the place of the continuation of M, shows graphically the convenience of going to a second working fluid. How far such saving will pay will be proved to us in Germany. There appears to be room for economy.

The turbine is under the same limit as regards pressure, in fact high pressures are perhaps even more difficult to use, and superheating does not, as already explained, seriously increase the mutivity of the heat taken in by the boiler. The turbine is almost perfect thermodynamically. There is practically no variation of temperature of any part of it, so there is no growth of entropy by conduction. Wire-drawing produces kinetic energy which is wholly convertible into work. There must be increase of entropy, however, through eddies and steam friction. In fact, one aim in design is to avoid the turbine being a large "porous plug." There is no admission condensation, and the increase of entropy of the steam passing through should prevent any expansion condensation. If superheating is necessary to prevent expansion condensation, it is the highest compliment to the designer, for it shows the wire drawing due to irreversible expansion and that due to the leakage, and the axial conduction of heat taken together are not enough to prevent expansion condensation, and the accompanying friction and entropy production near the exhaust end. Comparatively little superheating decreases not only the water, but the heat consumption of a Parsons turbine.

One of the chief disadvantages of steam engines for stations with small load-factors, is the difficulty of storing energy so as to get uniform boiler load. Batteries are no longer used for this, and the difficulty reduces the value of steam in comparison with the gas engine. Mr. Druitt Halpin has proposed, and used "Thermal Storage." Lagged vessels are filled with water raised to the temperature of the working steam. This arrangement, however, is not isothermic; that is to say, to get out the energy the temperature must fall. What is wanted is a reservoir containing something which undergoes a physical or chemical isothermal change. For instance, a substance that fuses at the right temperature, and has a high latent heat of fusion, or a substance which like sulphur changes allotropically with considerable change of internal energy, at a suitable temperature. Unfortunately, there is no substance within the range of practical enigneering. Moreover, the storage is on the wrong side of the engine. To store heat with a mutivity of only some 0·35 is not so promising as to store some higher form of energy. The secondary battery thus begins with an apparent advantage. The difficulty of storage is another drawback to the steam engine, and gives the gas engine a further advantage.

Before leaving the steam engine it may be in place if I bring before the Institution a recommendation of the Institution of Civil Engineers that in future the British Thermal Unit be written B.Th.U. instead of B.T.U., as B.T.U. is used for "Board of Trade Unit."

The Gas Engine.

There is no other comprehensive name that covers the type of engine worked by gas and oil. The combustion need not be internal, and perhaps will not be internal in the future, but in a sense all are *worked by gases.*

The simplest ideal form is a machine that pumps a small volume of air under a high pressure into a furnace, and draws out a large volume of gases at the same pressure and a very high temperature. The engine should then expand this gas down to the temperature of the air. The limits of the gas engine are essentially constructive, and the difficulties in the way of large gas engines are enormous. Theoretically the gas engine has a very great advantage. The possible range of temperature is so high that the mutivity approaches unity. In addition to this the combustion, whether inside the engine or not, is very efficient. But a reciprocating engine cannot work at the temperature of burning carbon, or hydro-carbons, so that furnace gas at, say, 2,000° A. cannot be led along pipes and used in a reciprocating engine. If the mutivity is sacrificed and the furnace gases diluted with cold air from the pump this plan is inferior. The high temperature can be used by burning inside a cylinder or explosion chamber cooled by water. The hot gas is then surrounded by a cool layer, but most of it is at a very high temperature. Then we come on two difficulties. First, if the gases are exploded there is great strain on all the working parts. Next there is difficulty about the expansion. It would be good to make the engine compound, but then the valves give trouble. The valve has to deal with a rush of gas, which in a compound engine would be little below the explosion temperature. Even with great expansion in a single cylinder there is much difficulty about the exhaust valve. Piston valves are alone used for steam at high temperatures, but for much higher temperatures such as the exhaust of a gas engine, a mushroom valve is generally employed. Opening a mushroom valve against the gas in a large engine means a matter of tons. There would clearly be difficulty in leading hot gases about through complicated valve passages to intermediate and low-pressure cylinders. All the same, when gas engines have the field to themselves and compete closely, the compound gas engine must come in. At present we waste a great deal of energy in the exhaust, and we have to make the cylinder large enough for the expansion, and strong enough for the explosion. Then as the exhaust is chemically different from the original mixture we must either halve the power by using the Otto or Rochas cycle, or we must adopt some other method of scavenging, or use an auxiliary compression pump. It is thus clear that though we begin with gas at going on for 2,000° A., with a mutivity at a lower temperature of the air 0·86, we have to exhaust at such a high temperature that the mutivity is, say, 0·4, or a little better than in the steam engine. If a certain range of temperature is available, it is better to have it at a low temperature, so that the mutivity is greater. Raising both temperatures is like hoisting a reefed sail higher without unreefing it. (Except in a heavy sea) the lower part of the sail is most efficient. We have thus in the gas engine a machine which from a thermo-dynamical point of view ought to be exceedingly good ; but the difficulties in building, especially very large engines to utilise the high possible mutivity, and saving by having the heat produced where used, reduce the efficiency of the gas engine enormously. In spite of that, the large gas engine seems likely to oust the steam engine for large powers during the next

few years. The best way to get a high efficiency out of a gas engine would probably be to make it compound, exhausting at a temperature suitable for raising steam. The steam engine would then exhaust at a temperature suitable for raising SO_2 vapour. But the chances are that Dowson, Mond, or other producer gas will be available at such low prices that the extra steam and dioxide engines would not pay for attendance, interest, and depreciation. With very cheap gas the first thing is to make big engines, the next to make them so that they never break down, and the last thing, to make them efficient. The gas engine may be, comparatively speaking, in the state Watt left the steam engine, but it will doubtless make very rapid advances, as it is in the hands of very competent and highly educated engineers.

I have said nothing about the oil engine, of which the Diesel may be

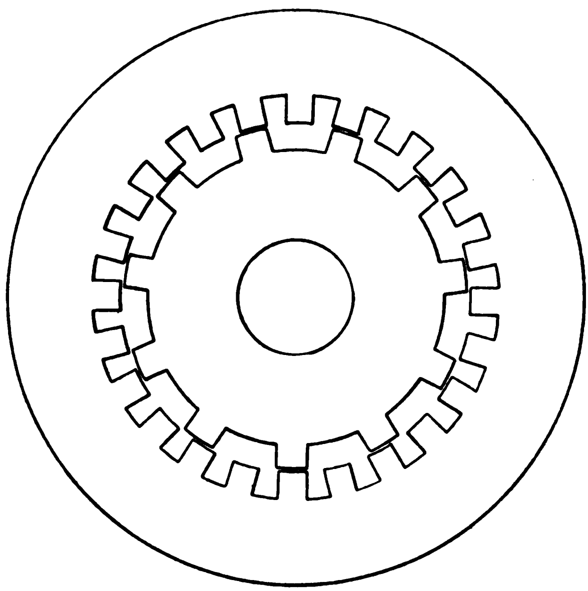


FIG. 2.—Rotary Field Direct-current Dynamo.

taken as the most prominent example. I have never seen or tested a Diesel, but see no reason why it should not be as claimed. There seems to be great prejudice against it, because it is novel. When people, especially English people, strongly condemn a new thing without giving specific reasons, it is safe to conclude that it is very good.

Dynamos.

As regards efficiency we have reached the practical limit already, *for further reduction* in dynamo losses would make no appreciable

difference in the total efficiency of a station. In fact we are rather following Continental practice in having slow-running machines with many poles, even for direct currents, and efficiencies are perhaps lower for large machines than in the best English practice of a few years ago. This is also true as regards output from a given size. We are not likely to make much advance in dynamos now, as we are limited on one hand by the hysteresis loss in iron, which prevents our using higher inductions in armatures, and low permeability which limits our field and armature tooth inductions. It does not seem likely we will now find iron much better in either respect. Nor are we likely to find a better available conductor than pure copper. As insulator we have mica. It looks, therefore, as if we were within sight of our limits in dynamo and motor designs.

Though dynamos are limited in speed, generally to that of the driving engine, it is possible to make a dynamo give an output corresponding to a much higher speed. In order to get several periods per revolution, alternating dynamos have perhaps from the beginning been made with a number of field and armature coils, so as to be in a sense a collection of elementary dynamos. Mr. Mordey showed how to make an alternator with only one armature and one field-magnet coil. Fig. 2 shows a way of getting a pressure corresponding to a high speed in a direct-current generator. The field has a number of N. and S. poles in succession, as usual in an alternator. The field is laminated. The armature is drum-wound as if for a two-pole field, and it has teeth which come opposite N. poles on one side of the machine, and S. on the other. A very small movement of the armature causes the positions of correspondence to change rapidly, so that the magnetic field rotates rapidly. Thus if there are 11 armature teeth and the speed is, say, 200 revolutions, the field rotates at 2,200, giving the corresponding pressure. If the field rotates with the armature the effective speed is 2,000; if the other way, 2,400. The difficulty in such a machine will arise from magnetic leakage. The armature reaction is also considerable. But reversing devices analogous to those proposed and used by Edison, Houston, Sayers, Atkinson, and others are available, and the machine may be made so that the armature largely excites the field in the case of a generator, and wholly excites it in the case of a motor. It is a question whether such a device as this may give us a light compact railway motor.

Transformers.

In alternating transformers there has been little room for improvements for the last ten years. The "ageing" of the iron was a trouble but now there seems little possible advance.

Secondary Batteries.

The secondary battery in central station work has been used as a store to equalise the load, and to reduce the running plant at the

times of heavy load. Owing to the high full-load station pressure with feeder systems, the station battery is generally for use at light loads only. But the secondary battery has for a long time been on the border of success for traction work, both on tramways and on the road, and a further improvement in batteries may be expected to produce very great changes in important branches of engineering.

The first question asked is, Why do we stick to lead? The answer is that the case is very special and other things will not do. We are practically limited to lead, at any rate in acid cells. Take first the plate that oxidises on discharge. It should not dissolve in the electrolyte, as if it does the deposition and solution will be uneven, and the plate will grow trees and come to grief. This puts zinc out of court, unless some electrolyte is used which gives some insoluble salt of zinc, which does not attack zinc on open circuit, and which gives a good electromotive force with it. Iron is out of court for the same reason; there is no suitable electrolyte. The strong organic acids such as trichloracetic or oxalic are apt to have their positive radicles split up by electrolysis, even if a strongly positive metal can be found with an insoluble salt. Lead is thus the only metal practically available in an acid electrolyte. Silver in hydrochloric acid would give no pressure, and the acid would be decomposed at the anode. On the other plate we need an insoluble depolariser, else a two-fluid cell must be used, involving a porous diaphragm, diffusion, and impracticability. Not only must the depolariser be insoluble, but it must be converted into an insoluble body on discharge. The coating must be a conductor in one state or the other, or there will be no proper contact. In the lead cell there is always enough peroxide and metallic lead in the coatings to secure electrical contact though the discharge product is an insulator. The depolarising coating must be connected to a conducting plate which is not attacked by local action. Lead and silver are the only available metals, and sulphuric, and perhaps phosphoric, the only acids, for the nitrate of lead is soluble, and hydrochloric acid is decomposed by lead peroxide. Lead is protected by its coating of sulphate, or peroxide as the case may be.[1] It thus seems as if we were limited almost absolutely to lead and sulphuric acid. It is wonderful that we have the lead cell at all. We owe it to the chance observation of Planté. The theory was not understood for a long time. For many years it was thought that the pressure was due to the PbO_2 and Pb changing into PbO. The acid was merely put in to make the electrolyte conduct, and sulphuric was used because people used it in gas voltameters, and they never thought that it ought to be as strong as practicable to give the pressure and output. The formation of lead sulphate was regarded as a difficulty to be overcome.

In the lead cell we want lightness, large capacity, cheapness, rapid discharge, efficiency, and mechanical strength, and durability. These qualities are mostly antagonistic. Large capacity means rapid deterioration. Mechanical strength means weight. It is thus no use testing a cell for capacity without testing the efficiency and durability too, and

[1] *See papers of the late* Gladstone and Tribe, and Drake and Gorham.

so on. Published battery reports are often misleading, because they omit essential information.

In an alkaline electrolyte such as caustic, such metals as iron, nickel, cobalt, and copper form oxides which are insoluble. The metals are thus electro-negative in caustic, like gold or platinum in acid. The electrolyte here acts merely as a conductor, as the dilute acid was supposed to act in the lead cell. The pressure is thus chiefly due to the change from metal to oxide on one plate less the reduction on the other, and is small. There may be a Gibbs-Helmholtz temperature coefficient pressure in addition. Though the pressure is smaller, the metals admit of light plates or grids, and the coatings may conduct in both states, instead of only one as in lead cells, so that a larger proportion of the coating may be active. The future of this type of cell is uncertain, as very little has been published as to results. Our limits in secondary batteries thus seem to be settled by the need of having insoluble electrodes and insoluble coatings.

Cables.

The conductor itself can hardly be improved, but there is great room for improvement in the insulation. It is largely the insulation of the cables that limits our pressures, and therefore our distances of transmission. For 1,000 kilowatt cables the cost is about a minimum for 8,000 volts; above that the cost of insulation increases faster than the cost of copper falls. It is exceedingly unlikely we have reached the limit in insulation. There is no branch of electrical engineering so important as cable making. Cables form a large portion of the capital outlay in large systems. Yet there is no branch of the industry which is run on less scientific lines. The days of secret mixtures known only to the workman who makes them may be passing away; but even now the whole art of cable-making is a question of trial and error, with a good deal of the last component. Engineers do not know now whether rubber is better than paper, nor can they tell what any particular make of cable will be like after ten years' use. We do not even know how to test a cable. Sometimes we test it as if it were a telegraph wire; at other times we test with twice the maximum pressure, and if it does not break down, we trust we have not injured its constitution, and put it down. Or we break down a little bit and assume it will all stand some proportion of the break-down pressure.

Apart from resistance to rupture and leakage, capacity and dielectric hysteresis are important factors. In alternating work capacity may produce very unexpected effects. In series with an arc, say at a switch, oscillation may be set up. When condensers were made at Teddington they were sent out with printed instructions to avoid switching off. They had to be switched off in shunt to or in series with a resistance. The loss of power by dielectric hysteresis has again been discussed recently; and whatever its exact amount may be in different cables, it is often a very serious factor. We do not seem to have arrived at anything like the lower limit of this yet.

Overhead bare conductors for very high pressure are not used in

this country. There is a fairly definite limit to the pressure available. When the fall of pressure, or dielectric stress just round the wire exceeds the breaking-down value of air, the air is "torn" and discharges take place which involve considerable loss. This can be reduced by increasing the size of the conductor. It may thus pay to use aluminium or even zinc, or a combination for very high pressure overhead power transmissions. If zinc falls in price much relatively to copper, it may come into use for bare conductors.

Condensers.

It is hardly worth while discussing condensers now, as there is generally excess of capacity on systems, so that the current leads relatively to the pressure; and there seems to be no demand for condensers. Condensers can be made which will hold their charge for several weeks without perceptible fall of pressure. Bringing the poles of a large station within a small fraction of a millimetre over an enormous area with nothing but a few thicknesses of paper between them does not appeal to an engineer. A more mechanical form of condenser might be made, as in Fig. 3, if it is ever wanted. A coil of copper or aluminium is held in a strong radial field so that it is free to move at right angles to the field. If the equation of motion is worked out it will be found to behave like a condenser on an alternating current. I do not know how it would sound. Some ten or twelve years ago I urged the use of over-excited synchronous motors for taking up idle lagging currents. This method is now used frequently, and it lessens any demand there might be for condensers.

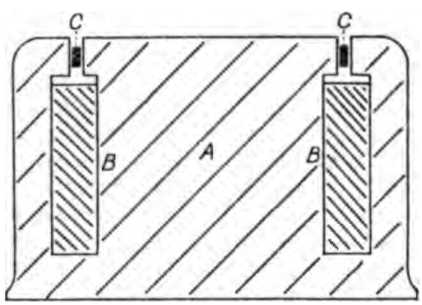


FIG. 3.—Electro-magnetic Alternating Condenser. A, field magnet; B, exciting coil; C, armature coil free to vibrate vertically in a radial field.

Light.

Our chief work, until lately, has been producing light. Here the inefficiency and waste is prodigious, and though it is mostly unavoidable, there is still great room for improvement. We take geat care over our stations, watching every penny from the coal shovel or mechanical stoker to the station meter. We quarrel over 1 per cent. in the generators. When we get to the mains we care less, and once we have got to the consumers' meters we care nothing at all.

Practically all light is wanted for use by the human eye. The *human eye is* exceedingly sensitive; it is calculated to see a distant

star when receiving 10^{-8} ergs per second, so that one watt would enable, say, five thousand billion people to see stars with both eyes, but it would have to be used economically. In reading a book the eye would need much more than this; and then, as the book radiates light in half of all directions, only a little is used by the eye, so even if all the light from a source were concentrated on a book there is enormous waste by useless radiation from the book. But the source of light does not illuminate only the book; the book probably subtends a small solid angle, so we have another source of waste. The eyes reading a book in a fairly good light want something of the order of two ergs per second, so that a watt would only work the optic nerves of, say, the inhabitants of London. But the book, say 200 square centimetres, would need about 3,000 ergs a second to illuminate it. A candle,[1] which gives a light of 4π, radiates about 0·2 watt or five candles a watt; that is to say at an efficiency of unity, we would get five candle-power or 20 units of light per watt. The efficiency of a glow-lamp is only about 0·25 candle-power per watt,[2] or 0·05, so there is room for improvement. The first thing, naturally, is to see what limits there are in the way of increased efficiency. The obvious goal is direct production of "light without heat," by which is meant producing only the rays of wave-lengths which affect the eye. The firefly appears to succeed in this. The radiation is obviously not that of a hot body, any more than the phosphorescence of jelly fish or microbes or phosphorus. It has been suggested that, though the radiation is not that of a hot body, it can only be produced at the efficiency which a hot body giving the same colour would give. Personal discomfort would prevent the vainest firefly from generating more than, say, 0·1 watt per square centimetre of cooling surface, and anyhow the insect appears to develop no appreciable heat.

There is no thermo-dynamical reason why electrical energy should not be converted directly into radiation of any wave-length without loss; I do not know if there is any molecular impossibility, but apparently our limits are practical—that is to say, it may be done, but we have not yet hit on the way of doing it. The vacuum tube appears to be a means of converting electric power direct into radiation. The Cooper-Hewitt lamp, for instance, gives an efficiency of about three candles per watt, or something like 0·6. All these figures as to light are a little vague. Unfortunately the light is of a very bad colour. It is very actinic, but the wave-lengths are too small. One method is to degrade the light by making it act on silk dyed with matters which lower the radiation to a redder colour by fluorescence.

The Arc Light.

The arc has been very fully studied in some directions, and not in others. Most makers of arc lamps seem to devote their whole attention to the mechanism, and look upon the arc merely as a hot gap that has to be preserved by suitable apparatus. Many lamp makers, on the

[1] See Note D, *p. 39*, "*The* Standard Candle."
[2] *See Note E, p. 39,* "*Inverted* Ratios."

other hand, have records of exhaustive experiments on the relations of the pressure, current, and light with different carbons; but they are very seldom published. On the other hand, an enormous amount of laborious experiment on such points as these is available,[1] and on the back electromotive force of the arc.[2] The physics of the arc, an exceedingly difficult branch of study, has not received much systematic attention yet. The crater of an arc is, no doubt, heated to the point of volatilisation of carbon at the pressure of the air. If other gases get at the crater, the vaporisation temperature would be less. (There is a small increase of pressure which I suggest is due to the electromagnetic effect of a current localised in a conducting fluid. This may be neglected.) The crater may be rough, as carbon, though it softens, does not melt before volatilising, and it may be merely speckled with points at its volatilising temperature, so that its brightness is not uniform. But there are so many anomalies about the arc that one cannot say anything definite with safety. For instance, if the temperature is limited by the vaporisation of carbon, what must be the specific heat of vaporisation of carbon? Where does the vapour go to, and what happens to it in an enclosed lamp? In condensing into smoke it should give light of the same colour as the crater. If it has an enormous specific heat, it ought to raise the other pole to crater temperature where it condenses. If it is a light gas, a large portion of its specific heat of vaporisation may go to external work. Most of the upper carbon is burnt away by external air; if a pencil to match the crater is volatilised it does not account for much power. If the vapour is very light, there must be large volumes from the upper carbon. Then what conducts? Carbon vapour alone, or mixed with a little monoxide or nitrogen, is a very good conductor at these temperatures. Does that go to show that carbon vapour dissociates like iodine or chlorine, etc.? The whole question of the physics of the arc deserves far more careful study than it has yet received, but the work is surrounded with difficulties and is really a branch of the theory of the passage of electricity through gases, a matter of the greatest scientific importance somewhat out of our way as practical electrical engineers. But as engineers in the broader sense we are as much interested in questions of recondite physics as of costs of generation.

Looking at the arc as analogous to a vacuum tube with no vacuum, but very hot and very rarified gases instead—a difference in degree, not in kind—the question arises whether we can get direct conversion into radiation without intermediate heating. The enclosed arc seems to give us something of this sort. It is difficult to see what goes on inside the inner globe, but the arc itself, apart from the crater, seems to give more light. The efficiency of an enclosed arc is much reduced, however, by the deposit and globes. Hot vapours, such as that of metallic salts in the Bunsen flame, give out light by direct action, not because they are hot, but by some chemical change, and this holds good of vapours in vacuum tubes, and doubtless also in the arc. In an

[1] See, for instance, *The Electric Arc*, H. Ayrton, a valuable epitome of the work done on the arc lamp up to the present time, including the authoress'.

[2] *See Note F, p. 39*, "Back Electromotive Force of the Arc."

enclosed arc the carbon vapour instead of combining with oxygen may first condense at the temperature of the crater, forming a luminous envelope round the arc itself. By mixing suitable salts with the carbons we may thus expect to get electrical power converted directly into radiation. It has long been known that adding sodium salts to the carbons increases the light, perhaps without improving it, but such experiments have been carried out rather empirically, and apparently without any distinct idea of direct production of light. Recently great attention has been excited by the Bremer arc lamp, which owes its effect to the addition of salts to the carbons. Another recent development is the arc between pencils of oxide, such as zirconia. This arc, which was tried, if I remember right, about 1897, has come to the front again. It may depend simply on high temperature, or on small amount of shadow from the lower electrode; or it may be that zirconia volatilises and condenses as a luminous cloud outside the arc, giving a light like burning aluminium or magnesium, or the hot vapour may also be giving the zirconium emission spectrum in addition. I have not seen this light.

To sum up as to the arc light, we do not seem to have reached our limit as to light from pure heating, because we lose a lot of light into the opposite carbon. Many attempts have been made to expose the crater freely. But, far more important than this, I would urge that the arc is not necessarily a hot body radiator only, but that it may also convert electrical power directly into light in the space between the electrodes, and this gives a chance of rising more nearly to our theoretical limit of about five candles per watt.

The Incandescent Lamp.

This simple hot carbon wire in a bulb involves the most extraordinary physical complexities. A great many curious things go on inside the simple-looking globe. A good account of what is known—especially since he took the subject in hand—has been written by Dr. Fleming,[1] and the scientific manufacture of this interesting article has been fully described by Mr. Ram.[2] The incandescent lamp is a simple hot body radiator, and the limit of efficiency depends chiefly on the temperature of the carbon. As we are limited by the size of mains, we can only use pressures of 100 volts or 200 volts, and this limits us to carbon, or something of still higher specific resistance. The high pressure is bad for the lamp in every way. The carbon has to be longer and thinner, and therefore weaker, while the great pressure between the ends of the carbons gives rise to invisible discharges across the hot interior. The high pressure thus means that the surface of the carbon is worn away quicker, and that the carbon is thinner and less able to stand it. Inherently a high-pressure lamp is worse than a low, but the convenience as to distribution outweighs this disadvantage. The limit of efficiency of incandescent lamps is chiefly due to the variations of supply pressure. Carbon is beginning to soften at ordinary lamp

[1] *The Physics of the Incandescent Lamp.* [2] *The Incandescent Lamp.*

temperatures, and the upper limit is soon reached. So that, thou; a lamp may give a third or even half a candle per watt when r steadily, it has to be rated at a quarter to make it safe under ordina conditions. The sensitiveness of the carbon lamp to pressure in i turn limits the practical variation of pressure of supply, and thus cos us very heavily in mains. If we had incandescent lamps which did n mind 20 per cent. pressure variation, we would have saved millions i mains in this country alone. Recently the demand for "ballast" fc the Nernst lamp has led to the introduction, for that purpose, of littl

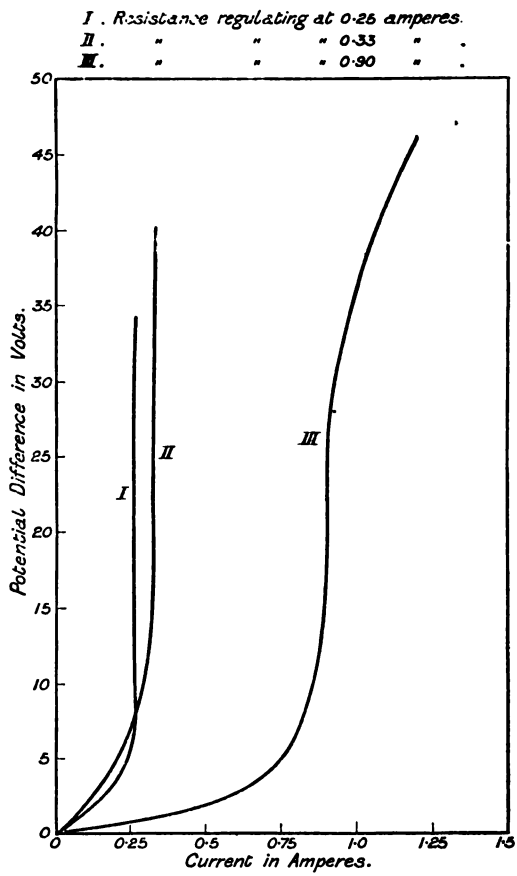


FIG. 4.—Pressure-current Curves for iron wire at critical point.

bulbs containing fine iron wire heated to the critical point. Tl "ballast" may be used in series with the incandescent lamp. Fig gives some curves taken by Mr. M. Solomon, who has been working *this subject.* It will be seen that a small resistance will allow a lan

to be run nearly at its maximum safe efficiency in spite of pressure variations, while a larger resistance will enable lamps to be run off tramway and other varying pressure circuits with good overall efficiency.

Other materials have been proposed instead of carbon. A great deal of work has been done on metals, but they have the great fault of low specific resistance. Even osmium seems unlikely for 200 volts. The specific resistance of metal goes up very quickly near the melting-point, but even then it is too low. I have worked a good deal at this problem, but unsuccessfully so far. Most of the metals generally called infusible are not nearly refractory enough. It is quite easy to melt such things as tungsten and molybdenum into globules at the temperatures necessary for high efficiencies. To oust the carbon lamp a great advance in temperature is necessary. I have found a metal that seems refractory enough, but it has too low resistance, and there are very serious difficulties in making wires of it. It would seem that black oxides ought to make good filaments. Most of these substances melt easily. They conduct when first made, and go on conducting when hot. But, if then allowed to cool, it will be found that they have undergone a change and no longer conduct. The black or brown oxide of vanadium is a good example of this.

The idea of making lamps of carbides has become very fashionable lately. People have put oxides into carbon for the last twenty years. The old idea is to get hold of an oxide that radiates more light at a given temperature than it ought to, which is itself a fallacy, while the idea of oxide in contact with carbon is chemically absurd. There is no oxide irreducible by hot carbon. The carbides are not by any means all refractory. Some are, though, but there are immense difficulties in making carbide lamps. I have made low resistance carbide lamps which stood high temperatures, but that is a very small step on the way. Mr. W. H. Story has gone on with this, beginning where I left off. After two years' solid work he has made no carbide lamps, but I believe he will eventually. To make a fine filament of an infusible material, which can be made only at electric furnace temperatures and which is generally decomposed by moist air, is not an easy task. It is easy to think you have made a carbide lamp by incorporating an oxide in the filament material, but the resulting filament is generally mostly if not wholly carbon. What happens to the metal under the circumstances is rather a mystery. There is, however, a chance of enlarging our limits in incandescent lamps of the ordinary kind, but it seems strange that the melting points of all known materials should suddenly reach a higher limit. Assuming the Stefan-Boltzmann law for ordinary light radiations, the fact that the efficiencies of refractory bodies all reach limits of the same order shows that the most refractory bodies melt at about the same temperature, somewhere in the neighbourhood of 3,000° A. As melting points are dotted along the temperature scale from 16° A. for hydrogen and something lower, not yet determined, for helium and bodies ending in "on" up to about 3,000° A., we might expect some to go up to 4,000, 5,000, and so on. Whatever the inter-molecular forces may be that bind the particles to make

solids, the vibration forces due to temperature seem to overcome the greatest at about 3,000.

Instead of an ordinary conductor, Nernst uses an electrolyte which stands a higher temperature. The conduction is electrolytic, as can easily be shown, but there are many curious phenomena, many of them so far unexplained, in the Nernst lamp. The efficiency of the Nernst lamp is about 0·6 candle per watt. It was at one time supposed to owe its efficiency to selective emission, but there is no reason to doubt that it is a pure temperature radiation. It might be said that as a vacuum tube and a Nernst glower both conduct electrolytically, both being transparent, the Nernst may work by direct conversion of electrical power into radiation: but it does not work in this way, at least not mainly. Emissivity seems to vary with temperature in the case of solids and liquids. A transparent, that is to say a non-absorbing, gas does not radiate when heated, but a transparent solid emits when hot even those rays which it does not absorb when cold. Recently some writers have treated the ordinary carbon filament as being transparent when hot. If it were transparent it would not follow the cosine law, but it also would have to conduct electrolytically. The idea of carbon conducting electrolytically is too extreme to be entertained. It is difficult to say how the idea of the transparency of hot carbon has arisen. Perhaps carbon does not behave as a "black body" because it is not quite black, and because its index of refraction is widely different from that of a vacuum, so that it may depart from the law of cosines and look brighter in the centre, as if transparent. The Nernst lamp, however, gets us nearer our limits both by high temperature and high pressure, so that it pushes out the limits of constant pressure distribution. One difficulty in the electrolytic lamp is to get the material to conduct at a low enough temperature, and to stand a high temperature too. The Nernst lamp is essentially zirconia, which stands a high temperature. A little basic oxide of the yttria group is added, and there is probably formed a portion of zirconate of these metals, which is fusible enough to conduct at a lower temperature. This subject needs elucidation, as the zirconates are probably like the silicates, forming a long series. Moreover, zirconia and yttria in the proportions for simple zirconate form a very infusible material. The whole subject is very obscure. We cannot say we have got anywhere near our limits of high temperature efficiency of running, low temperature in starting, or high pressure in the electrolytic lamp.[1]

Electric Heating.

The limit of electric heating is clearly purely financial. To convert heat into other energy with a very small efficiency and to send it out by expensive cables and then to degrade the energy down to heat again is obviously much dearer than burning coal or gas direct. But in many domestic cases the convenience is so great that the limit is not so low as

[1] See Note G, p. 41, "Efficiency and Temperature."

might be thought, and electric heating for cooking and other domestic uses may develop considerably. The electric arc and incandescent lamps are essentially cases of electric heating. By far the most important use of electric heating is the furnace. Here the temperature available is only limited by the volatilisation of the electrodes, and this enables us to get temperatures otherwise unavailable, so that we can get chemical actions which are impossible at lower temperatures, either because they are endothermic or because the materials do not come into chemical contact at ordinary temperatures. It is impossible to say what our limits are in the electrical furnace. Probably the temperature is limited by the volatilising of carbon. The products are not limited to endothermic compounds, the furnace is useful for the reduction of metals and phosphorus; and for melting glass and, it is hoped, silica for optical and laboratory purposes, and perhaps for cooking utensils and evaporating pans and crucibles in chemical engineering and metallurgy.

Railways.

It is almost absurd to begin to consider the limits of the use of electrical transmission on railways at this date. The future of electric railways, electric tramways, and automobiles is rather a matter of vague conjecture and picturesque prophecy. Tubes are multiplying rapidly, and railways are putting down electric transmission on suburban lines in Europe and the States. On short lines with many stops we have to contend with inefficiency at starting. On long lines there is difficulty of transmission or cost of transformation and difficulties of collection. We are very much handicapped, or limited by the want of a suitable variable speed-gear for large powers. For short lines the ordinary tramway system is used and many schemes have been proposed. Three-phase motors, either in cascade with high primary pressures, or arranged simply, are being tried and have many advantages. Various arrangements of motors have also been discussed or tried. Series distribution has been proposed but never tried. Though it superficially resembles series tramways it differs essentially in the switches being a long way apart, and not worked by the train, and having constant current variable speed generators. One difference makes it practical as far as the switches go, the other efficient. But a variable speed-gear would solve the difficulty very much better than any of these schemes. We could then have simple alternating circuits with all the consequent ease of transmission and transformation, and the trains could go at suitable speeds with high efficiencies throughout. The Leonard is an electrical variable speed-gear, but it is expensive and not very efficient. A few mechanical gears have been proposed to work with synchronous alternating motors. The most obvious arrangement is a system of pumps. Air pumps with a reservoir have been proposed. A system of oil pumps of variable stroke is also in use to some extent for automobiles.[1] But machinery of that sort practically means that an electromotive must

[1] *See Note H, p. 41,* "Variable Speed Gear."

have not only a motor of say 1,000 kw., but what amounts to two sets of motion work. This must mean serious expense. We may therefore say that we are limited by the want of either a variable speed simple alternate-current motor, or a simple variable speed-gear capable of transmitting a very large torque, and packing into an engine. A recently developed scheme is the use of low-frequency alternating currents with laminated series-wound motors. This solves the difficulty, but at the expense of large idle current, induced pressure in short-circuited armature coils, large expensive and inefficient transformers, and the ordinary disadvantages of the series-motor on constant pressure. This plan is well worth serious study.

The collection of large currents at great speeds has long loomed as a limit. The published accounts of experiments at Zossen would lead us to suppose there is no trouble on this score. Still it is a difficulty many engineers fear.

In electric tramways there is no limit in sight. The power can be sent over any distance desired, and there seems to be no limit to the people who want to travel on electrical trams. The question of electrolysis is rather that of a limit to the duration of pipe companies' property. It is a very difficult question. Though the threatened effects of electrolysis have no doubt been exaggerated it is at best a question of degree, and the ingenuity of engineers is continually reducing the chance of damage. It has recently been urged that frequent reversals of polarity of the system reduces the electrolysis very considerably.

Electrolysis.

This is a branch of industry in which it is very difficult to tell our limits. In electrolytic copper-refining our limit is that of the copper wanted. Our electrolytic industries suffer mostly from the limits of intelligence of the investing public. It is assumed that we cannot do electrolysis in England because we have no water power. This is only an excuse for inactivity. As already explained, we can do just as well without water power. A blast furnace is much more valuable than a waterfall of similar power, because it is near coal and in an industrial district. Moreover, as already explained, the cost of electrical energy is a small portion of that of most electrolytic products. At first electrolysis was to be applied to copper-refining. Then to caustic soda. The output of electrolytic caustic is really rather limited by the demand for bleach. What is urgently wanted is some other way of storing and carrying chlorine. Steel bottles and compression plant are an unsatisfactory solution. What are the limits in the way of electrolysing fused salt? They are all incidental limits. The containing vessel is a difficulty. Sodium vapour attacks all silicates. Sodium distils near the temperature of fused salt. If not volatilised it forms a conducting bridge from the cathode. It attacks iron, though slowly. Hot porcelain and earthenware conduct electrolytically—as, by the way, the maker of electric frying-pans knows—hot chlorine attacks metals, even when dry, and hot carbon cannot be exposed to the air. In addition sodium and perhaps chlorine are soluble in hot salt, and traces of

sulphate in the salt act as carriers as sulphate and sulphide. I could a tale unfold if I read out laboratory notes of sodium experiments on a fairly large scale. The difficulties are all incidental, though, and I have little doubt electrolytic sodium at a few pounds per ton will be in the market soon, and will affect profoundly many chemical and metallurgical industries. I would like an opportunity to continue experiments myself, but others will, I feel certain, soon succeed.

In metallurgy electrolytic solution processes are in use or on trial for the more valuable metals, such as copper and nickel. The reaction between chlorine and metallic sulphides at high temperatures brings the whole domain of sulphide ores under our sway. Thus a sulphide, say galena, is treated with chlorine, which gives off the sulphur as sulphur, which is condensed and sold, making chloride of lead. The silver is extracted by stirring with a little lead, and the fused salt is then electrolysed, yielding pure desilverised lead and chlorine. The process is thus self-contained, yielding sulphur, lead, and silver. It is specially applicable to mixed refractory ores which are now nearly valueless and very plentiful, and contain much metal content, such as the mixed lead-zinc sulphides of America or Australia. These reactions have been proved on the large or ton scale, and there is no technical difficulty. Unfortunately mine people are somewhat ignorant of electrical matters, and it is exceedingly difficult to get them to understand or appreciate a process like this, capable though it may be of paying good dividends on very large capitals indeed.

In all these metallurgical extractions we may roughly take the cost of energy as a farthing per kilowatt-hour for steam, and half that for gas. Allowing a rough average pressure per cell, we may take it that electric energy costs £100 per tonne- or ton-equivalent by steam and £50 by gas. That would be £3 a ton for zinc, £1 for lead, £3 for copper, and iron by steam, and half these figures by gas power. This means that the metallurgy of all the sulphides, except perhaps iron, is within our grasp. It may pay to make a pure iron, free from phosphorus, silicon, manganese, and carbon at something under £10 a ton from pyrites ores (which may also contain zinc, nickel, copper, etc.), and then add exactly the desired amount of other constituents or "physic" to produce with accuracy steels of special grades.

But our limit in electrolysis in this country is almost entirely human inertia. Commercial and financial people do not understand it, and fight shy of it. But our technical people are nearly as bad. The pure physicist, as a rule, takes no interest in electrolysis or physical chemistry, and thinks it belongs to the chemical classroom on the other side of the passage. The chemist thinks it is higher mathematics and will have none of it, the mathematician thinks it may be an exercise in differential equations ; but they are all agreed that it is a sort of Continental fungus which flourishes with no roots, and that it is beneath the attention of a scientific man to know enough about it to give a reason for the broad statement that it is all nonsense.

I have now tried to bring before you the various barriers which appear to bar our progress in various directions : it is for you to get over those you can, and to get round the rest.

Note A.—MUTIVITY.

The term motivity was introduced by Kelvin (*Phil. Mag.* 1879, Math. and Phys. Papers, I.), but is not used much now. The expression "Available energy" is well known and clear, though through an obvious slip in the first edition of Maxwell's "Heat" it was called, but not confused with, "entropy." The term Motivity suggests,

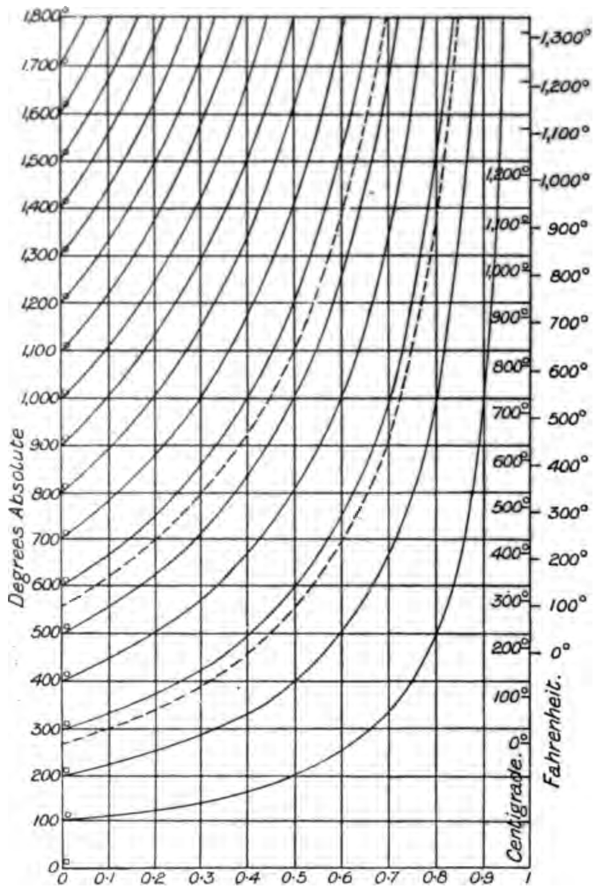


FIG. 5.

according to the modern fashion in nomenclature, a specific quantity, but I have no right to use it in a new sense. I therefore suggest "mutivity," which is a contraction for "mutativity," to denote the changeability or convertibility of the heat into other forms of energy. The mutivity is thus a number equal to $(\theta_1 - \theta_0)/\theta_1$, which is less than unity, so that the mutivity is the available energy per joule. The energy integral of the mutivity is thus the available energy. The *curves Fig. 5 give* roughly the mutivity corresponding to various

temperatures. Thus if one wants to find the value of some heat at, say, 600° absolute with a lower limit of 200, the line starting from 200 is followed till it cuts the horizontal 600° line. The other ordinate gives the mutivity 0·66.

Note B.—Common Sense Scientific Units.

To talk of boiler pressures in megadynes per square centimetre may seem strange. We, the electrical engineers, have an almost perfect scientific system of units so that the quantities hang together in a sensible way. No one who once understands the C.G.S. system ever wants to work any other. If a man never wants to apply thermodynamics to anything but British steam engines, and never wants any foreigner to read his work, and never wants to know what is done outside this small island, the English system may suit him if his time is of so little value that he does not mind a tangle of useless coefficients. If, however, any one wants to do quantitative work—and all such work should be quantitative—the ordinary units and tables in thermodynamics are terrible. In England we measure heat in thermal units with no name, and energy in foot-pounds, and power in horse-power. Our thermometer is graduated without any apparent relation to anything. It has been said that its zero is the temperature of ice-cream, and its 100 that of the fevered patient who consumed it. Mr. Ram has pointed out that each degree really corresponds to the expansion of mercury by a ten-thousandth part. Our pressures are in pounds per square inch less the magic number 14·6967, but we take condenser pressures as the difference between the number of inches of mercury read and 29·92. These measurements involve a huge amount of unnecessary arithmetic every time they are used. This means waste of time and chance of inaccuracy. The French are by no means beyond reproach. Their pressures are either in atmospheres, atmospheres less one, or in millimetres of mercury. The Centigrade scale is just as absurd as the Fahrenheit. Even with 273 added, it has no numerical relation with anything. Some day we will perhaps have an absolute scale. But practically all thermodynamical workers fail to realise that heat is really energy, and have a special unit of heat depending on the specific heat of water which has nothing to do with anything else. We, as the most scientific of the engineers, ought to use our own scientific and rational units, so that the horse-power, foot-pound, pound per square inch, poundal, calorie, British thermal unit, inch of vacuum, and all the thousand and one silly weights and measures may be helped into the limbo of disuse.

Note C.—Entropy.

There is an unfortunate misconception as to the nature of the function entropy in most treatises on the steam and gas engine, and in the use of the θ,ϕ diagram. Clausius defined entropy so that—

$$\int \frac{dH}{\theta} < \phi.$$

The whole idea of entropy is that in every change in nature it must increase, otherwise the change cannot occur. In the hypothetical but impossible case of reversibility—

$$\oint \frac{dH}{\theta} = 0,$$

where the brackets mean that the integration is round a closed cycle,

and—
$$\int \frac{dH}{\theta} = \phi.$$

The study of reversible changes occupies most of the space in books on thermodynamics, just as the study of frictionless mechanisms throws light on engineering problems; but, though it is numerically correct in the limiting and purely hypothetical case of reversibility, the equation

$$\int \frac{dH}{\theta} = \phi$$

as a definition of entropy is fundamentally wrong. It gives a wholly wrong notion of entropy. The temperature entropy diagram was, I believe, first brought forward by Gibbs (Trans. Connecticut Acad. II. 1873, p. 309). This paper is difficult to get in English, but a translation is accessible in "Thermodynamische Studien," Gibbs, pub. Engelmann. Here the author is definitely dealing mainly with hypothetical reversible processes and the properties of fluids, and such things as perfect gases, and any one reading the paper alone would get a wrong idea of entropy; but it was rather meant to clear the ideas of students, and did not affect engineers. We really owe the practical use of the entropy diagram, and the insight we have gained through it in spite of the confusion as to entropy, to Macfarlane Gray. In his papers, "The Ether-pressure Theory of Thermodynamics" and "Rationalisation of Regnault's Experiments" (Proc. Inst. Mech. Engin. 1889, p. 379), the definition of entropy is as faulty as Gibbs'; but he again is discussing the properties of steam and dealing with reversible processes, so again there is no numerical error. In discussing the θ,ϕ diagram of an engine, however, it is usual to define the entropy by the equation $d\phi = dH/\theta$, which is wrong, and to lay down that the area of the θ,ϕ is equal to that of the p,v diagram (divided by the mechanical equivalent of heat). This incorrect definition has no doubt given rise to the notion that entropy is the factor of heat corresponding to temperature. I think Zeuner first called entropy "heat weight," a confusion of thought which is constantly cropping up. The whole object of engine analysis is to trace irreversible changes. If the diagrams were of the same area, after multiplying by the wholly unnecessary coefficient, the engine would be reversible and perfect, and there would be no use in investigating it. The badness of an engine in its way of using its working fluid should come out in terms of the excess of the θ,ϕ over the p,v diagram. In thermodynamics there is a constant tendency to confusion between the working substance and the reservoirs. Entropy is a function that essentially concerns the reservoirs. Thus a perfect engine would allow no increase of entropy. If an engine and a boiler were perfect, the entropy taken in by the water from the hot gases would be equal to that given to the condenser water. There are various increases of entropy in practice. The most important are increases of entropy due to adding feed-water below the boiler temperature, wire drawing steam, heating the air by convection and radiation of steam pipes and engine, conduction between cylinder walls and steam, conduction through cylinder walls from jacket, sudden expansion into condenser, and mechanical friction throughout the mechanism. The importance of each growth of entropy depends on the temperature. The temperature integral of the irreversible increase of entropy would be an area on a real θ,ϕ diagram. The total area of the θ,ϕ diagram would then exceed that of the work of the engine (or of the p,v diagram if the mechanical friction is excluded) by this area, which would represent the badness of the engine. To give a clear idea of the value of this loss the mutivity

should come in as a coefficient right through. This is done graphically by cutting off the bottom of the area. We should therefore localise all increases of entropy in an engine, and then try to prevent them.

It may be urged that the definition of entropy with which I find fault is given not only in engineering text-books, but that it occurs in nine out of ten treatises on mathematical physics. That is so; most writers define entropy incorrectly. As the mathematical treatment of reversible processes naturally occupies most of their attention, an incorrect definition gives no quantitative error until irreversible processes are considered. The mathematical physicist then generally, perhaps consciously, rises to accuracy, but sometimes he does not. Adiabatic and isentropic are thus also defined as synonymous. The isentropic or curve of constant entropy does not coincide with the adiabatic or curve of no passage of heat except in hypothetical reversible changes. On a p, v diagram the adiabatic curve may be anywhere between the isentropic and the isothermal coinciding with either in a limiting case. Unfortunately also entropy fell among pedagogues. The pedagogue takes $d\phi$ as a leading illustration of an exact differential. It is extraordinary that out of the whole domain of physics he should select the one differential whose conspicuous characteristic is that in nature it never is exact. Perhaps he has a fellow feeling for it. It is sometimes stated that if the quantity is a single valued function of the co-ordinates, its differential is exact, and that is what exactness means, which is also inaccurate. This not only shows that it is infinitely easier to work with mathematical symbols than to get a clear grasp of their physical meaning, but it emphasises the extreme difficulty and slipperiness of thermodynamical work in particular.

Note D.—The Standard Candle.

The standard candle, which ought to give a light of 4π, is about as absurd as the horse-power. The candle and the horse are about equally nearly obsolete, and the candle is about as likely to give a candle-power—or 4π units of British light—as a horse to give a horse-power. The horse has one advantage over the candle: he is not inextricably mixed up with the 4π controversy, and well-meaning people do not try to rationalise him as a unit.

Note E.—Inverted Ratios.

There is a curious tendency among engineers and other scientific men to get ratios wrong. People talk of efficiencies in "watts per candle"; "pounds of steam per horse-power-hour," a specially barbarous unit; insulation in megohms per mile: specific resistance in microhms per cubic centimetre; and muzzle velocity in foot-seconds; while elasticity is defined so that perfectly elastic means absolutely rigid. The "candle foot" and "candle metre" or "carcel metre" are now coming in to add to the unnecessary inaccuracy and confusion. It is sincerely to be hoped that we soon have light units in terms of watts and temperature of radiation, so as to fit into the C.G.S. system.

Note F.—Back Electromotive Force of the Arc.

Ohm's law, $C = E/R$, is really a statement of a physical fact, namely, that if the other physical conditions remain constant, the ratio of C and E is invariable. It is not a mere definition, though it is a definition too. But if the physical conditions alter with variations

of C, there is only left a definition of R as being equal to E/C. We cannot by any measurements of E and C find out anything about the nature of R. If we choose we may write $C = E/R + e/R$, where e is defined as a pressure which may be negative, and R is defined as a resistance. Any attempt to determine R and e from measurement of C and E is merely an attempt to solve a single equation with two unknowns, which is absurd. If two or more sets of readings of E and C, E^1 and C^1 and so on are taken, a fancy definition of R and e may be given, so that $C = (E + e) R$, $C^1 = (E^1 + e) R$, etc., and if many readings are approximately consistent with constant values of e and R they may be called electromotive force and resistance, but they are only fancy names, and have no physical meanings. If many readings are inconsistent with constant values of e and R some qualifications may be given to them, but still there is no physical knowledge obtained. All the measurements of E and C in the world can only give E and C; we may give any names we like to functions of E and C, but they give no further knowledge. They are really round-about methods of stating the values of E and C. The back electromotive force and resistance of the arc are thus, from this point of view, mere matters of fanciful definition. A huge amount of labour has been devoted to trying to determine the resistance and back pressure of the arc in terms of E and R. I would urge that all this is an attempt to solve a problem which does not exist, and the waste of time and trouble is due to looseness of thought in not clearly defining the terms "resistance" and "back electromotive force" in cases where Ohm's law is no longer a law stating that a certain physical quantity is not varied by changes of current, but a definition, which if accepted as $C = E/R$, gives R merely as a ratio of C and E, or if modified to $C = (E + e) R$ involves two unknowns in one equation. Not only have innumerable experiments been made measuring C and E, but their ratios of relative increase are taken as if they gave further information. This involves exactly the same fallacies. Many of the methods involve making a change, say in C, and assuming the arc has not had time to change accordingly, but the arc is too quick. The various ingenious arrangements with alternating or telephone currents superposed on direct, or direct superposed on alternating are of the same type. They combine the argument in a circle as to the definition, with an attempt to deceive the arc by taking measurements before the arc has time to feel the changes due to change of current. These considerations are urged with the view of possibly saving unprofitable work. The first thing to do before trying to determine back electromotive force is to settle very clearly and definitely what you mean by back electromotive force, and by resistance. If they can be given in terms of any measurable quantities other than E and C, those other quantities are to be measured. But if they are only functions of E and C there is no use trying to solve one equation with two unknowns, and one is merely working back to his own definition, and not making a physical research. I urge this with diffidence, but at the same time with vigour, because we have an awful example before us in the "Seat of the Electromotive Force" in a cell. If people had started with a clear physical definition of what they meant by the seat of electromotive force, and if they had agreed as to a definition, not only in words but in idea, there would have been neither research nor controversy. We use the terms "resistance" and "electromotive force" so familiarly that we naturally assume we know what we mean by them. But that by no means follows.

From Ohm's law as a statement of a physical property of matter we get to regard resistance as a property in accordance with which $dH/d\theta = C^2R$; that is to say, resistance has come to mean a property

by which electrical energy is degraded directly into heat, an irreversible process, while an electromotive force with a current means reversible change of electrical or mechanical or other power, or *vice versa*. This difference, though I have never seen it formally stated, runs tacitly through science. Again, we may regard electromotive force as being produced only by lines of induction cutting the circuit, to take the crude conception. This is the same definition in another form; except that in the reversible interchange between chemical and electrical energy, magnetic induction is not generally considered. The behaviour of magnetic induction due to the movements and chargings and dischargings of ions has not been worked out in any publication as far as I know, but it ought to be. Thermo-electricity is worthy of study from the same point of view.

Taking these definitions and going back to the arc, it is clear that nearly all the power is spent at the crater. The drop of pressure may therefore be taken as being at the crater, so that the arc proper is nearly at the same pressure as the other carbon. If the change of electrical energy is directly into heat, as there is no reason to doubt, then it is due to resistance and not to back electromotive force. On the other hand, the radiation from the arc itself is probably due to direct conversion of electrical power into radiation; that is to say, the gas does not radiate light because it is hot,—gases at 3,000° C. do not radiate any light,—but because the current affects the particles in such a way that they produce light. There are thermodynamical reasons for treating radiation as heat, but as the energy is not in this case first degraded to heat to heat the gas, and then radiated because the gas is hot, the radiation is caused not by resistance, but by back electromotive force. The molecular movements, whatever they may be, involve magnetic induction increasing or decreasing in the interlinked circuit in such a way as to produce a back pressure, the power spent in overcoming this back pressure going out as a continuous stream of radiation. This back electromotive force must be very small—nothing of the order of 40 volts for instance.

Note G.—EFFICIENCY AND TEMPERATURE.

It is necessary to point out that the view that the efficiency of a radiating body depends on temperature only, and not on the surface, is not generally held by scientific men. Some eighteen years ago I believe I was a minority of one in holding that in an incandescent body, such as a lamp filament, the efficiency depends on the temperature only; and that the colour of light depends on the temperature only. Mr. Ram, in his book on the Incandescent Lamp, holds this view, but he is an old assistant of mine in lamp making; and we are still a small minority of two or more. My reason for holding this view was that it seemed to me that if a body with a special surface gave out light of a whiter or bluer colour, the phenomenon would be at variance with the second law of thermodynamics, and the surface would be doing the work of Maxwell's demon, not by letting through only the most rapid molecules, but letting through only the vibrations corresponding with them, which is much the same thing.

Note H.—VARIABLE SPEED GEAR.

A variable speed gear has been invented by Mr. Hall for use in automobiles; but his gear is also applicable to electromotives. It consists of two sets of oil pump engines of variable stroke mounted in

a rotating frame. One set of oil pumps works on to a fixed axle, the other on to the driving axle, and the motor drives the frame round. If the frame goes at constant speed, the speed of the driving axle varies according to the strokes of the pumps. If they are equal, the engine goes at half speed. If the fixed axle pumps are at no stroke, and the moving axle at full stroke, the engine goes at full speed. The speed can thus be gradually varied from nothing up to full speed, and at full speed the efficiency is 1. At half speed the pumps are doing their

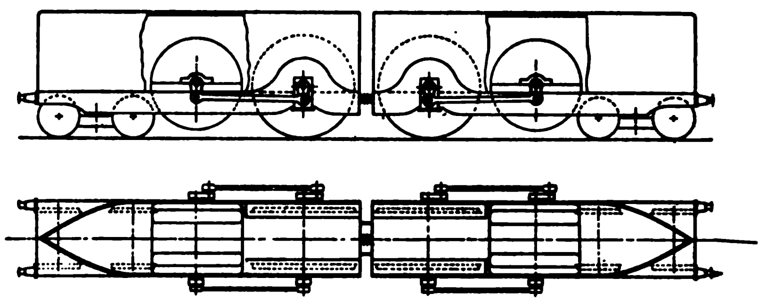


FIG. 6.

maximum power. If their efficiency is 0·9 the total efficiency is 0·95, and so on.

Mr. Hall purposes to use two gears and four motors. The driving axles are connected with the driving wheels by coupling rods. The electromotive is articulated in the middle to turn corners better. This

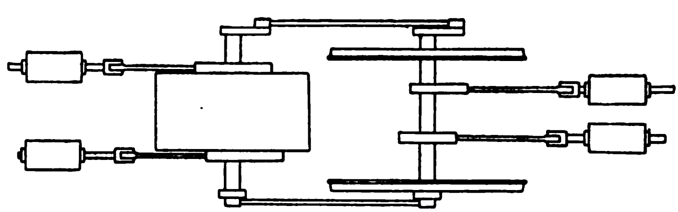


FIG. 7.

system allows the train to be run by high-pressure single-phase, constant-speed motors. At starting, on say double torque, the current is practically zero, climbing up to full current at half speed. The change is then made from constant torque to constant power, and the train gradually gets up to full speed with its normal current. In ordinary running the speed would be about nine-tenths of the maximum, to allow a margin, so that the oil pumps would transmit 0·1 of the actual power, or about 0·08 of the maximum power. Taking the pumps' efficiency to be 0·9 the loss is 0·008, or under one per cent. at ordinary speeds. This system thus allows the electromotive to exert any starting torque the motion work is strong enough to transmit, or the adhesion to utilise, while the motor only takes in enough power to run the mechanism round; and it allows the electromotive to run at any speed within designed limits, taking just the power needed; all this being done on

a single-phase alternating-current system of convenient frequency, with every facility for transmission over great distances and distribution, and energy return on stopping. The Hall electromotive is shown diagrammatically in Fig. 6.

The electric motor permits of another arrangement, however, in which the pumps do not rotate. The motor armature is on the driving axle, and the field magnets can revolve too. The field magnets work two stationary oil pumps with variable stroke. The oil works two more oil pumps which act on the driving axle, also with variable stroke. This mechanism has the same economical results as the Hall gear. It is shown in Fig. 7.

Mr. ALEXANDER SIEMENS: It is my privilege to-night to move,—"That the best thanks of the Institution be accorded to Mr. Swinburne for his most interesting Presidential Address, and that, with his permission, the address be printed in the Journal of the Proceedings of the Institution."

It is hardly necessary for me to add to the words of the motion, and I therefore simply move that the best thanks of the Institution be given to him for his address.

Mr. S. Z. DE FERRANTI formally seconded the motion, which was carried with acclamation.

The PRESIDENT, in reply, said: Gentlemen, I thank you very heartily indeed for the exceedingly kind reception you have given to my poor address, and for the great attention with which you listened to what I am afraid was a rather long, very dry, and technical address.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected :—

Associate Members.

Vittorio Giovanni Lironi.	Alejandro Voglino.

Associates.

Wm. H. Govier.	Frank Russell Seller.
George T. Rayner.	Frederick Turnbull.
Arthur Allen Saward.	George Arthur Webb.

Student.

George Wharton Hellicar.

The Three Hundred and Eighty-first Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, November 27th, 1902—Mr. J. SWINBURNE, President, in the Chair.

The minutes of the Ordinary General Meeting held on Thursday, November 13th, 1902, were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that their names should be suspended in the Library.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Associate Members—

John Henderson Mackail.

From the class of Students to that of Associates—

S. L. Cazeaux.
Donald Albert Hills.
Francis Ernest Pring.

Donations to the *Library* were announced as having been received since the last meeting from The Director-General, Indian Government Telegraphs ; to the *Building Fund*, from Mr. T. Cushing ; and to the *Benevolent Fund*, from Mr. W. H. Patchell, to whom the thanks of the meeting were duly accorded.

Messrs. D. H. Kennedy and A. Russell were appointed scrutineers of the ballot for the election of new members.

The PRESIDENT : I have now the pleasure to call on Sir Oliver Lodge to read his paper. But before doing so, I would remind this Institution that the last time we had the pleasure of hearing the lecturer he was Professor Lodge : now he is Sir Oliver Lodge. It makes no difference to us at all. We have known him for a long time, and no honour of this sort could raise him in the least in our estimation. But at the same time it must be remembered that it is a great honour. To estimate the value of such an honour as knighthood, you must multiply the knighthood by the man who receives it, and in a case of this sort we all know what the value is, because here it is. I now call on Sir Oliver Lodge to read his paper on "Electrons."

ON ELECTRONS.

By SIR OLIVER LODGE, F.R.S., Vice-President.

INTRODUCTION.

In Maxwell's *Electricity* published in 1873, section 57, the following sentence occurs in connection with the discharge of electricity through gases, especially through rarified gases :—

> "These and many other phenomena of electrical discharge are exceedingly important, and when they are better understood they will probably throw great light on the nature of electricity as well as on the nature of gases and of the medium pervading space."

This prediction has been amply justified by the progress of science, and no doubt still further possibilities of advance lie in the same direction. The study of conduction through liquids first, and the study of conduction through gases next, combined with a study of the processes involved in radiation, have resulted in an immense addition to our knowledge of late years, and have opened a new chapter, indeed a new volume, of Physics.

The net result has been to concentrate attention upon the phenomena of electric charge, and greatly to enhance the importance of a study of electrostatics. Not long ago Fitzgerald used chaffingly to speak of electrostatics as "one of the most beautiful and useless adaptations of nature"; and it was becoming the custom with teachers who felt that they must attend exclusively to the practically useful, and not waste their students' time on decoration and superfluities, almost to ignore, or at any rate to scamper through, the domain of electrostatics, and to begin the study of electricity with the phenomena of current, and especially of the connection between electricity and magnetism.

And certainly from the severely practical point of view, as well as from many other aspects, this part of electrical science remains the most important; but to him who would not only design dynamos and large-scale machinery, to him who in addition to the training and aptitude of the engineer possesses something of the interests, the instinct, and the insight, of a man of science, to such a one the nature and properties of an electric charge, at rest and in motion, constitute a fascinating study; for there lies the key to the inner meaning of all the occurrences with which his active life is so intimately concerned—there lies the proximate solution of problems which have excited the attention and taxed the ingenuity of philosophers and physicists and chemists since men began to escape from the struggle for bare existence—that most immediately practical of all occupations—and felt free to devote themselves, some to art, some to literature, some to the

accumulation of superfluous wealth, and some to the gratuitous pursuit of speculation and pure theory.

Your President, Mr Swinburne, realising that a society like this was sure to contain men eager to pursue their subject into some of the intricacies far removed from their immediately practical occupations, wrote and pressed me to come up and give an explanatory sketch of what had been done of late in the world of pure science towards the elucidation of the most familiar electrical processes; and with but little hesitation I consented, feeling sure that what the President urged would not be regarded by the Institution at large as out of place or unsuitable, though of my own motion I should never have thought of offering any such paper.

PART I.

First I must lay a basis of pure theory: we must consider the properties of the ancient and long known phenomenon called an electrified body.

Two substances placed in contact and separated are in general united more or less permanently by lines of force, the region between them being in a state of tension along the lines and of pressure at right angles. These lines have direction: they begin at one body and end at another, they map out a field of electrostatic force, and their terminations on one one or other of the bodies constitute what we call an electric charge. Electric charges are of two kinds, one corresponding to the beginning of the lines, the other to their ends. To one class of bodies, called insulators, the lines appear rigidly attached; whereas in another class they slip easily along, and are transferred from one such conducting body to another in contact with it, with great ease.

The tension in the lines tends to bring the ends together as near as possible, giving rise to what is observed as electrical attractions and repulsions.

In empty space it is probable that the only way of destroying such a field of force is to allow the two bodies to approach each other, and thus shorten up the lines to nothing; though even so it is not probable that the charges are destroyed, but only approach so close that they have no external effect at any moderate distance. When matter is present, however, it may be able to assist this collapse of the lines in various ways, giving rise to the various phenomena of conduction and of disruptive discharge.

If one of the two oppositely charged bodies is sent away to a considerable distance, while the other is isolated and regarded alone, the lines of this latter start out in all directions in nearly straight lines, giving rise to the simple notion of a single charged body,—a thing which is no more really possible than is a single magnetic pole. The other ends of the lines must be somewhere, though they may be so far away as to be spoken of as, for all practical purposes, at infinity.

Now consider how far this field of force belongs to the body, and how far it belongs to space, that is to the ether surrounding the body. *The body is the nucleus* whence the lines radiate, but the lines them-

selves, the state of tension and other properties which they represent and map out, do not belong to the body at all; at each point of space there is an electric potential, and this potential represents something occurring in the ether and in the ether alone.

A Charged Sphere.

Picture in the mind's eye such a charged body, say a charged sphere, and let it change its position; how are we to regard the effect of the displacement on its field of force? Nothing in physics is more certain than this, that when a body moves, the ether in its neighbourhood does not move. The ether, in fact, is stationary: it is susceptible to strain, but not to motion; it is the receptacle of potential, not of locomotive kinetic energy.

The effect of the motion of the body, then, is to relieve the strain of the ether at one place and to generate it at another; the state of strain travels *with* the body, but *through* the ether.

Regarding the matter from the point of view of the ether, we might say that the field of force is constantly being destroyed and regenerated as the body moves. Regarding it from the point of view of the moving body, we should say that it carries its field with it.

The question now arises—and it is far from being an easy question—what sort of occurrences go on in the ether when this decay and regeneration of an electrostatic field is occurring, or when a field of force is moving through it? Can it adapt itself instantly to the new conditions, or does it require time? This matter has been studied, closely and exhaustively, by Mr. Oliver Heaviside.

Fix the eye upon a point a mile distant from the body; does the information about the motion of the body reach that point instantaneously, so that all the lines of force move like absolutely rigid spokes, every part simultaneously: and if so, how is the communication carried on so that the distant parts of the medium can be thus instantaneously affected? Or does the disturbance only arrive at the distant point after the lapse of a small but appreciable time; in other words, has there to be an adjustment to the new conditions—an adjustment which reaches the nearest parts first and the further parts later; and if so, what additional phenomena can be observed during the unsettled period?

The answer is that during the motion of the charged body, and even after the cessation of its motion, until the disturbance has had time to die away and everything to settle down into static condition again, the phenomena of *magnetism* make their appearance: a new set of lines of force quite different from the electrostatic lines (although they, too, exhibit a tension along them and a pressure at right angles) come into temporary being. These do not originate at one place and terminate at another: they are always and necessarily closed curves or rings, and in the present simple case they are circles all centred upon the path of motion of the charged body. At any point of space there are now three directions to consider: (1) there is the original direction of the electrostatic field—the original electric line of force; (2) there is the direction of the motion—that is, a direction parallel to the movement

of the charged sphere; and (3) there is the direction at right angles to these two; this last being the direction of the magnetic lines of force—the direction of the magnetic field.

I spoke of the magnetic field as temporary, but that is on the assumption that the charged body is merely displaced, moved from one position to another; if it is not stopped, but keeps on moving, then the magnetic lines continue as long as the motion lasts. Its strength at any point r, θ, is—

$$H = \frac{e u}{r^2} \sin \theta.$$

If we are asked whether such a magnetic field is weak or not, I have to reply that that depends entirely on how strong the charge is and how quickly it is moving. There is, in my opinion, no other kind of magnetic field possible; and so if ever we come across a magnetic field which we feel entitled to consider "strong," we must conclude that it is associated with the motion of a very considerable charge at a velocity we may properly style great. But certainly it is true that for any ordinary charged sphere moving at any ordinary pace—even supposing that it is a cannon-ball shot from the mouth of a gun—the concentric circular magnetic field surrounding its trajectory is decidedly feeble. Feeble or not, it is there, and to its existence we must trace all the magnetic phenomena of the electric current.

For just as there is no electrostatic field save that extending from one charged body to another, so there is no electric current except the motion of such a charged body, and no magnetic field except that which surrounds the path of this motion.

The locomotion of an electric charge *is* an electric current, and the magnetic phenomena surrounding that current are believed to be the only magnetic phenomena in existence. If any other variety is possible, the burden of proof rests on those who make the positive assertion.

One more statement:—

While the charge is stationary everything is steady, and we have an electric field only.

While the charge is moving at constant speed the current is steady, and we have a steady magnetic field superposed upon a steadily moving electric field, and a certain conveyance of energy in the direction of the motion.

But what about the intermediate stages, the stages of starting and stopping; what is the condition of things after the charge has begun to move but before it has attained a constant speed, and again when the brake is applied and the speed is decreasing, or when the direction of motion is changing? What phenomena are observable during the epoch of acceleration or retardation of speed or curvative of path? Something more than simple electrostatics and simple magnetism is then observed.

We get the phenomenon of induction—the generation of an induced E.M.F., of value at any point equal to the rate of change of the lines of *magnetic force* there. There being no conductor, this E.M.F. will

propel no current, but it will represent an electric force which was not there before, and in a new direction, perpendicular to the direction in which the growing magnetic lines are moving, which is outwards from the charge. Consequently the new or induced E.M.F. points in the direction of motion, though in the sense opposed to any change in it; and the effect of its superposition upon the magnetic field is to cause a certain small transmission of energy in a radial direction out and away from the accelerated charge. Some energy therefore flashes away with the speed of light, though in ordinary cases an exceedingly small amount.

It is from an electric charge during its epochs of acceleration or retardation that we get the phenomenon called radiation; it is this and this alone which excites ethereal waves, and gives us the different varieties of *light*.

The energy radiated per second is $\dfrac{2\mu e^2 \dot{u}^2}{3v}$,

where v is the speed of light and $\dot{u}$ is the acceleration of the charge e.

Thus, or rather by means of a very extensive development of these fundamental ideas, are all the phenomena of electricity and optics summarised, and, so to speak, accounted for.

Electric Inertia.

Whatever a charge may be, and whatever the hydrodynamic constitution of the ether, it must be able to maintain electric lines and magnetic lines, and to transmit energy wherever both sets of lines cross at right angles.

An accelerated charge is equivalent to a changing current, for $\dfrac{dC}{dt}$ may be written $\dfrac{d^2Q}{dt^2}$. Whenever a current changes we have an E.M.F. of self-induction set up equal to $L\,\dfrac{dC}{dt}$.

Considered from the point of view of a current constituted by a moving charge, this corresponds to a mass acceleration.

And the electrical acceleration is opposed by the E.M.F., just as the acceleration of matter is opposed by its mechanical inertia. The coefficient of the electric acceleration represents, therefore, an inertia term, and is properly called electric inertia.

By Lenz's law the effect of induction is always to oppose the cause which produced it. In the present case the cause is the acceleration or retardation of the moving charge, and so in each case this is opposed by the reaction of the magnetic lines generated by it.

Motion is opposed while it is increasing in speed, and it is assisted while it is decreasing in speed—an effect precisely analogous to ordinary mechanical inertia;—and therefore force is necessary, and work must be done, either to start or to stop the motion of a charged body. An extra force, that is, by reason of its charge. Whatever the inertia the body may have, considered as a piece of matter, it has a trifle more by reason of its being charged.

The value of this imitation or electrical inertia for the case of a charged sphere of radius a is

$$\frac{2}{3}\frac{\mu e^2}{a}. \text{ (See Appendix.)}$$

Since this is very important, I repeat :—

Just as a changing magnetic field affects an electrostatic charge, that is to say generates a feeble field of electric force, into the intensity of which the velocity of light enters squared in the denominator, so it is with a changing electric field, it generates a magnetic field proportional to its velocity of change; and if it is being accelerated, the magnetic field itself varies, and in that case generates an E.M.F. which reacts upon the accelerated moving charge, and always in such a way as to oppose its motion—by what is called Lenz's law, or simply by the law of conservation of energy : for if it assisted the motion, the action and reaction would go on intensifying themselves until any amount of violence was reached.

The magnetic lines generated by a rising current, that is by a positively accelerated charged body, react back upon the motion which produced them in such a way as to oppose it. To oppose it actually or elastically, not passively or sluggishly as by friction. The reaction ceases the instant the motion becomes steady: it is not analogous to friction therefore, but to inertia; it is the coefficient of an acceleration term.

The magnetic lines generated by a falling current, that is by a negatively accelerated or retarded charged body, react oppositely and tend to continue the motion: thus here also we have a term corresponding to inertia. And the charged body may be said to have momentum by reason of its charge while it is moving. The value of the momentum is proportional to the velocity, so long as the velocity is not excessively great, and accordingly the inertia term is constant, and independent of speed, under the same restriction. It may therefore be considered to be in existence even when the charge is stationary, and thus it simulates exactly the familiar mechanical inertia of a lump of ordinary matter.

In an Appendix will be given the simplest form of the quantitative relations here indicated, and the inertia due to an electric charge will be calculated. It is to be understood that whatever inertia a material sphere may possess, considered as matter, it will possess more when it is charged with electricity, and this no matter whether the charge be positive or negative. The amount of extra or electrical inertia is proportional to the electrostatic energy of the charge: that is to say, it is proportional to the charge and its potential conjointly. Call the charge e, and the radius of the sphere a, the potential will be $e/\kappa a$ and the appropriate inertia is $m = \frac{2}{3v^2} e \,.\, e/\kappa a$, where v is the velocity of light.

Another way of putting it is to say that if a mass of this amount were moving with the speed of light, its kinetic energy would be half as great again as the potential energy of the electric charge when

standing still; for $\frac{3}{4} m v^2 = \frac{1}{2} e \cdot \frac{e}{\kappa a} = \frac{1}{2} QV =$ potential energy.

Now any appreciable quantity of matter, even a milligramme, moving with the speed of light, has a prodigious amount of energy; namely, for the mass of one milligramme, fifteen million foot-tons. Or as Sir William Crookes has expressed it: a gramme, or fifteen grains, of matter, moving with the speed of light, would have energy enough to lift the British Navy to the top of Ben Nevis.

Consequently the inertia of any ordinary quantity of electric charge must be exceedingly minute. Notwithstanding this, it is quite doubtful whether or not there really exists any other kind of inertia. The question whether there does or not is at present, strictly speaking, an open one; though to my mind it is practically closed.

The only way of conferring upon a given electric charge any appreciable mass is to make its potential exceedingly high, that is to concentrate it on a very small sphere.

A coulomb at the potential of a volt has an electrostatic energy of half a Joule, that is $\frac{1}{2} \times 10^7$ ergs.

The mass equivalent to this would be

$$\frac{2}{3} \frac{10^7}{9 \times 10^{20}} = \frac{2}{27} \times 10^{-13} \text{ gramme} = 10^{-8} \text{ milligramme.}$$

Raise the potential to a million volts, and the mass equivalent to a coulomb at that potential would be the hundredth part of a milligramme: still barely appreciable therefore.

The charge on an atom as observed in electrolysis is known to be 10^{-10} electrostatic units. If this were distributed uniformly on a sphere the nominal size of an atom, viz., one 10^{-8} centimetre in radius, its potential would be one hundredth of an electrostatic unit, or about 3 volts. The energy of such a charge would be 10^{-12} erg, and the inertia of a body which would possess this energy if moving at the speed of light would be 10^{-33} gramme.

But this is incomparably smaller than the mass of a hydrogen atom, which is approximately 10^{-25} gramme. Consequently the ionic charge distributed uniformly over an atom would add no appreciable fraction to its apparent mass.

If, however, the atomic charge were concentrated into a sphere of dimension 10^{-13} centimetre, its potential would be 1000 electrostatic units or 300,000 volts, its energy would be 10^{-7} erg, and its inertia 10^{-28} gramme, or about $\frac{1}{1000}$ of the mass of a hydrogen atom.

All this is a preliminary statement of undeniable fact: that is to say of fact which follows from the received and established theory of Electricity, whether such things as electrons had ever been found to exist or not.

All that we have stated is true of an ordinary charge on any ordinary sphere which can be made to move by mechanical force applied to it.

It gives us the phenomena

of electrostatics when at rest,
of magnetism when in motion,
of radiation when started and stopped,

and it incidentally, by reason of the known laws of electromagnetic induction, exhibits a kind of imitation inertia, and in that way simulates the possession of the most fundamental property of matter.

I will add a few more closely connected assertions. Apply a sufficiently violent E.M.F. to a charged sphere, and the charge may be wrenched off it.

Insert an obstacle in the path of a violently moving charged sphere so as to stop it mechanically with *sufficient* suddenness, and again it is possible for the charge, or something like it, to be jerked off it and passed on. But to do this the speed of the sphere, as well as the suddenness of stoppage, must be excessive. Usually the charge is merely thrown into an oscillation, when the sphere is suddenly stopped; and it then emits a solitary wave or spherical shell of thickness equal to the diameter of the sphere: or greater than that diameter by the amount the sphere has moved during its retardation. When the acceleration is moderate, however, the radiation is less energetic and also less intense: less energetic because its power depends on the square of the acceleration, less intense because it is spread over a thicker ethereal shell. Röntgen rays are perceptible only when the speed was great and the stoppage so sudden that the wave or pulse shell is strong and thin.

The doctrine of the behaviour of a charged sphere in motion, and the calculation of the value of the quasi inertia of an electric charge, was begun by Professor J. J. Thomson in an epoch-making paper published in the *Philosophical Magazine* for April, 1881—one of the most remarkable physical memoirs of our time.

The stimulus to this investigation was supplied by those brilliant experiments of Crookes, published in the Philosophical Transactions for 1879, which were preceded by observations of Plucker and Hittorf, and followed by other observations by Goldstein and Puluj and others in 1880.

In 1891 Sir William Crookes was your President, and in his inaugural address expounded further some of these brilliant experimental investigations, to which Schuster and many others had contributed. It is not too much to say that up to the time of Crookes the phenomena of the vacuum tube were shrouded in darkness, notwithstanding much laborious and painstaking work done both in this country and on the Continent in connection with them; but that since the researches of Crookes in the seventies, the theoretical luminosity of the vacuum tube has steadily increased, until now, as Maxwell predicted, it is shedding light upon the whole domain of electrical science, and even upon the constitution of matter itself.

APPENDICES TO PART I.

APPENDIX A.

Calculation of the Inertia of an Electric Charge.

Let a spherical conductor of radius a carrying a charge of electricity e move forward with moderate speed u; meaning by moderate speed

anything distinctly less than the speed of light; it constitutes a current element of magnitude $e\,u$, and its circuit is closed by displacement currents in the surrounding dielectric; for its lines of force arise in the medium in front and subside in the medium behind, and so a displacement of electricity takes place from fore to aft to compensate the motion forward, and the lines of displacement are identical with the magnetic lines due to a short magnet. A charge may be said to travel carrying its electrostatic lines with it, or it may be said to be constantly generating a radial electrostatic field in front and destroying one behind. When an electric field thus moves partly laterally it generates a magnetic field—in the present instance in circular lines round the line of motion—for the moving charge is an element of a linear current.

The generation of these magnetic lines acts so as to oppose the current which produced them, but so long as they continue steady they exert no effect on it. When they subside, however, they tend to prolong the current which maintained them. Consequently, if the moving charge (or current) tries to stop, its retardation meets with obstruction; it is constrained to persist by the subsidence of the magnetic field which its motion excited and maintains. Its velocity is not resisted, there is nothing equivalent to friction, but its acceleration + or — is obstructed, an effect precisely analogous to inertia. If it is at rest it will need force to start it, and if it is in motion its motion will persist.

The charge acts, therefore, as if it had inertia, and we can proceed to calculate its amount.

While moving it is a current and will be surrounded by rings of magnetic force, whose intensity, at any point with polar co-ordinates $r\theta$ referred to the line of motion as axis and the moving charge as origin, will be the quite ordinary expression (with $e\,u$ for the current element instead of $C\,d\,s$)—

$$H = \frac{e\,u \sin\theta}{r^2}.$$

The ordinary expression for the electrostatic force at the same point is—

$$E = \frac{e}{K\,r^2};$$

and if the motion is slow this value will be preserved, but if it is rapid the electric field gets weaker along the axis and stronger equatorially, having been shown by Mr. Heaviside (*Philosophical Magazine*, April, 1889) to be given by the following expression—

$$E = \frac{e}{K\,r^2} \cdot \frac{1 - (u/v)^2}{\left\{1 - (u \sin\theta/v)^2\right\}^{\frac{3}{2}}},$$

where v is the velocity of light.

The strength of the magnetic field will be similarly modified in this case; but the simplest mode of stating it is to express it in terms of E, and to say that *always*—

$$H = K\,E\,u \sin\theta,$$

The rate of transmission of energy will be the vector product of E and H; and the whole magnetic energy, that is the whole energy due to the current, *i.e.*, due to the motion, will be obtained by integrating the ordinary expression $\mu H^2/8\pi$ all over space outside the charged sphere, viz., from a to ∞ all round. In the general case this expression is a little long, but in the most important case, when the speed of motion u is decidedly less than the speed of light v, it is quite simple, and the working may as well be given:

$$\left.\begin{matrix}\text{Kinetic}\\ \text{energy}\end{matrix}\right\} = \int_a^\infty \frac{\mu H^2}{8\pi} d(\text{vol.}) = \frac{\mu e^2 u^2}{8\pi} \int_0^\pi \int_0^{2\pi} \int_a^\infty \frac{\sin^2\theta}{r^4} dr \,.\, rd\theta \,.\, r\sin\theta d\phi$$

$$= \frac{\mu e^2 u^2}{8} \int_0^{2\pi} \int_a^\infty \frac{\cos^2\theta - 1}{r^2} dr \,.\, d\cos\theta = \frac{\mu e^2 u^2}{3a}.$$

Comparing this with mechanical kinetic energy $\frac{1}{2} m u^2$, we see that the charge on the sphere confers upon it additional kinetic energy, as if its mass were increased on account of the charge by the amount—

$$m = \frac{2\mu e^2}{3a},$$

which may also be written—

$$m = \frac{2}{3} \frac{\mu K \,.\, e^2}{Ka} = \frac{2}{3v^2} \,.\, e \,.\, \frac{e}{Ka} = \frac{2}{3v^2} \times \text{charge} \times \text{potential},$$

or—

$$\frac{3}{4} m v^2 = \text{the electrostatic energy of the charge.}$$

In other words, the mass equivalent to the charge is such that if it were a piece of matter with constant inertia travelling at the speed of light, its kinetic energy would be half as great again as the potential energy of the electric charge when standing still.

APPENDIX B.

The Electric Field due to a Moving Magnet.

If a short bar magnet or uniformly magnetised sphere (its moment M being the intensity of magnetisation × the volume of the sphere) moves along axially, that is in the direction of its magnetisation, with velocity

u, it generates circular lines of electric force all centred upon its axis, much as a moving charge generates circular lines of magnetic force. If there is a conducting path around any such circle, then the motion of a magnet along its axis will generate a current in it, but if there be no conductor the motion will only result in an electric displacement which subsides when the magnet stops.

The intensity of the magnetic field at any point along the axis is well known to be $2\,M/r^3$; at any point on its equatorial plane it is $-M/r^3$; and in any intermediate direction it is, as regards magnitude alone—

$$H = \frac{M}{r^3} \sqrt{\left(1 + 3\cos^2\theta\right)}.$$

All this holds for the moving as for the stationary magnet, provided its speed does not approach that of light.

The electric force at the same point is—

$$E = \frac{3}{2}\frac{M\,u}{r^3}\sin 2\,\theta$$

$$= 3\,H\,u\,\frac{\sin\theta}{\sqrt{(4 + \tan^2\theta)}}.$$

The electrostatic energy resulting will be the integral of $K\,E^2/8\,\pi$ everywhere outside the moving magnetised sphere of radius a, viz.—

$$\text{Energy} = \frac{K}{8\,\pi}\iiint\left(\frac{3\,M\,u}{r^3}\right)^2 \sin^2\theta\cos^2\theta\,d\,r\,.\,r\,d\,\theta\,.\,r\sin\theta\,d\,\phi$$

$$= \frac{K\,M^2\,u^2}{5\,a^3} = \frac{M^2}{5\,\mu\,a^3}\left(\frac{u}{v}\right)^2$$

The displacement acts like an elastic strain set up in the dielectric, storing the above energy statically; and so long as the magnet continues moving steadily the electric displacement exerts no force upon it; but acceleration will be resisted. If the magnet begins to go faster it sets up more displacement, and the act of setting this up constitutes a transient current, which opposes the motion as long as the acceleration continues, but dies out the instant the motion becomes steady again.

Conversely if the motion of the magnet began to slacken, the electric strain would begin to subside, and its subsidence would constitute an inverse transient current which would assist the motion *i.e.*, oppose the slackening. In other words, the variations of the circular electric strain in the surrounding medium confer upon a moving magnet a spurious or apparent momentum, in addition to its real mechanical momentum; and thus the elastic strain itself may be said to represent a spurious or *apparent inertia due to magnetisation*, in addition to any real mechanical

inertia which the body holding the magnetism may itself possess. And the amount of this extra inertia is—

$$m = \frac{2}{5}\frac{K M^2}{a^3} = \frac{2}{5}\frac{M^2}{\mu a^3 v^2} = \frac{8}{15} \cdot \frac{\pi I M}{\mu v^2}$$

$$= \frac{2}{5} \frac{H_0 M}{v^2},$$

where I is the intensity of magnetisation, and H_0 the intensity of the field, inside the substance of a uniformly magnetised sphere of radius a and magnetic moment M.

The equivalent mass moving with the velocity of light would therefore have an energy equal to one-fifth of the potential energy of the magnetised sphere if it were held at right angles to a field of its own internal intensity.

This result may be applied, *mutatis mutandis*, to a moving molecule consisting of a pair of equal opposite electrons not in absolute coincidence.

PART II.

Discovery of the Atom of Electricity.

Quoting again from the great Treatise of Clerk Maxwell, 1st Edition, we find on page 312, in the chapter on electrolysis, the following sentence :—

> "Suppose, however, that we leap over this difficulty by simply asserting the fact of the constant value of the molecular charge, and that we call this constant molecular charge, for convenience in description, one molecule of electricity."

Thus some idea of the conception of the atomic nature of electricity was forced upon men of genius by the facts of electrolysis and a knowledge of Faraday's laws. But Maxwell went on, after a few more paragraphs :—

> "It is extremely improbable that when we come to understand the true nature of electrolysis we shall retain in any form the theory of molecular charges, for then we shall have obtained a secure basis on which to form a true theory of electric currents, and so become independent of these provisional theories."

It is rash to predict what may ultimately happen, but the present state of electrical science seems hostile to this latter prediction of Maxwell. The theory of molecular charges looms bigger to-day, and has taken on a definiteness that would have surprised him.

The unit electric charge, the charge of a monad atom in electrolysis, *whatever else it is*, is a natural unit of electricity, of which we can

have multiples, but of which, so far as we know at present, it is impossible to have fractions.

I will extract the following sentence from Section 32 of *Modern Views of Electricity*:—

> "This quantity, the charge of one monad atom, constitutes the smallest known portion of electricity, and is a real natural unit. Obviously this is a most vital fact. This unit, below which nothing is known, has even been styled an 'atom of electricity,' and perhaps the phrase may have some meaning. . . . This natural unit of electricity is exceedingly small, being about the hundred-thousand-millionth part of the ordinary electrostatic unit, or less than the hundred-trillionth of a coulomb."

The atom with its charge is called an ion. The charge considered alone, without its atom, was called by Dr. Johnstone Stoney an electron or natural electrical unit.

What we learn with great accuracy from electrolysis is the ratio of the charge to the mass of substance with which it is associated. It matters nothing how much substance is chosen, whether 100 atoms or one, whether an atom or a gramme or a ton, the amount of electricity associated with it in electrolysis and liberated when the substance is decomposed, increases in the same proportion; the ratio is constant, and if determined for one substance is known for all.

This is the ratio which is technically known as the "electrochemical equivalent" of the substance. In the light of Faraday's laws, if this quantity is measured for one substance it is known for all, because the charge is the same for every kind of atom up to a simple multiple; and hence in specifying electrochemical equivalents there is nothing to consider but the atomic weight or combining proportion of the substance. Thus the electrochemical equivalent of oxygen is 8 times that of hydrogen, that of zinc is $32\frac{1}{2}$ times, and that of silver 108 times that of hydrogen. The substance chosen for a determination of the electrochemical equivalent may be the one which can be most accurately experimented on, and Lord Rayleigh has shown that such a substance is nitrate of silver, and has ascertained that if a current of one ampere is passed from a silver anode to a platinum cathode through a nitrate of silver solution, the cathode gains in weight by 4·025 grammes every hour. Hence the electrochemical equivalent of silver is

$$\frac{4{\cdot}025 \text{ grammes}}{1 \text{ ampere-hour}};$$

the electrochemical equivalent of hydrogen, being $\frac{1}{108}$ of this quantity, is—

$$\frac{4{\cdot}025 \text{ grammes}}{108 \text{ ampere-hours}} = \frac{4{\cdot}025}{108 \times 360} \text{ c.g.s.} = {\cdot}0001035 \text{ c.g.s.} = \frac{1}{96600} \text{ grammes per coulomb.}$$

Hence the ratio of an atom of electricity to an atom of hydrogen is $9{,}660\,\mu^{-\frac{1}{2}}$ c.g.s. *units, or approximately* $10^4 \sqrt{\left(\frac{\text{centimetres}}{\mu \text{ grammes}}\right)}$; the un-

known constant μ necessarily making its appearance because we are comparing quantities measured in different ways, viz., Electricity and Matter (see Appendix D).

The numerical part of this quantity is known with comparative exactitude,[1] that is to say up to the limits of error of experiment. To proceed further, we must make an estimate of the mass of an atom; that can be done, and has been done, in many ways, and we have been taught both by Dr. Johnstone Stoney and by Loschmidt, and notably by Lord Kelvin, that the mass of an atom of water is approximately 10^{-24} of a gramme, wherefore an atom of hydrogen will be approximately 10^{-25} gramme; whence the unit of electric charge is 10^{-21} c.g.s. magnetic unit, or 10^{-10} of an electrostatic unit or 10^{-20} of a coulomb.

I have emphasised this matter of the ratio m to e or e to m because it plays a considerable part in what follows. The absolute values are of less consequence to us than the ratio, and are only known approximately, but the ratio is known with fair accuracy, and the ratio for hydrogen is very nearly 10^4 magnetic units, or more exactly 9,660.

Thus what we learn from electrolytic conduction briefly summarised is that every atom carries a certain definite charge or electric unit, monads carrying one, diads two, triads three, but never a fraction; that in liquids these charges are definitely associated with the atoms, and can only be torn away from them at the electrodes; that the current consists of a procession of such charges travelling with the atoms; the atoms carrying the charges, or the charges dragging the atoms, according to from which point of view we please to regard the process.

Conduction in Gases.

We will now leave liquids and proceed to conduction by rarified gases, that is to say to the phenomena seen in vacuum tubes. If a long glass tube, say a yard long and two inches wide, with an electrode at each end, and full of common air, is connected to an induction coil and attached to an air-pump, the ordinary spark-gap of the coil being, say, two or three inches wide, we find that for some time after working the pump the electric discharge prefers the inch or two of ordinary air to a long journey through the partially rarified air in the tube, but that at a certain stage of exhaustion, one which any rough air-pump ought to reach, this preference ceases. A flickering light appears in the tube readily visible in the dark, which very soon takes on the appearance of red streamers like the Aurora Borealis, and then the sparks outside in the common air cease, showing that the rarified air is now the better conductor and the preferable alternative path. Let the exhaustion proceed further, and the axis of the tube becomes illumined with the glow, which is now much brighter, showing a band or thread of current, while the original spark-gap may be shortened down gradually to one-eighth of an inch, or even less, without any spark taking place

[1] The decimal places are correctly printed above; though the fact that 1 coulomb or 1 ampere-second is one-tenth of a c.g.s. unit, owing to the volt having been stupidly defined as 10^8 instead of 10^9, always stands ready to *introduce confusion* and error.

across it, showing that the rarified air is now a very good conductor When the best conducting stage is reached the tube is filled with a glow, called the positive column; and both ends of the tube are apt to look alike. If we exhaust still further—and to exhaust even as far as this something better than an ordinary air-pump is necessary, an oil or mercury pump being the most suitable—the column of light is seen to fill the whole tube, to gradually lose its bright red or crimson tint, and to break up into a number of very narrow discs like pennies seen edgeways. At the same time the spark-gap must be widened to something more like a quarter or half an inch to prevent the discharge from taking that path, and a dark space near the cathode now begins to be visible, the cathode itself being covered all over with a glow, while the anode is usually only illuminated at a point or two. The striæ into which the positive column has been broken up thicken and separate as exhaustion proceeds. The dark space near the cathode also enlarges, driving as it were the positive column before it into the anode, and looking as if it would presently fill the tube; but before it can do this it is noticed that the glow on the cathode itself is coming off as a kind of shell, leaving another dark space, a narrower and much darker space, inside it. The first dark space has been called Faraday's dark space; the second is generally known by the name of Crookes' This second dark space now increases in thickness, pushing the glow before it as the vacuum gets better and better; but the terminals of the spark-gap must now be pulled still further apart, else the discharge will prefer to take a reasonably long path through the air. Exhausting further still, the glow all disappears and the second dark space fills the whole of the tube; and now is noticed a new phenomenon, the sides of the glass have begun to glow with a phosphorescent light, the colour of the light depending on the kind of glass used, but generally in practice with a greenish light; a result evidently of being the boundary of the dark space. If exhaustion proceeds further, the resistance of the tube becomes very high, and the spark may prefer to burst through an equal and ultimately even a greater length of ordinary air. This is the condition of the tube so much investigated by Crookes, by Lenard and Röntgen, and by many other observers. It is the phenomena occurring in this dark space which have proved of the most intense interest.

Cathode Rays.

So far we have supposed that the cathode is a brass knob or other convenient terminal introduced into the tube; but if we now proceed to use other shapes, as Crookes did, using a flat disc or a curved saucer-shaped piece of metal, and if we then introduce into the dark space various substances, we shall find that the dark space is full of properties which are most clearly expressed by saying that it is a region of cathode rays—that is to say, of rays or something as it were shot off from the cathode. There is evidently something being thus shot off, which, however, is invisible until it strikes an obstacle, something which seems to fly in straight lines and to produce a perceptible effect only

when it is stopped. Such a something might be a bullet from a gun, which is quite invisible when looked at sideways, but may produce a flash of flame when it strikes a target, or may do other damage. So it is with these cathode rays: the region of their flight is the dark space; the boundaries of that space where the projectiles strike are illuminated. A substance with phosphorescent power, such as many minerals, or even glass, phosphoresces brightly, and the path of the rays can be traced by smearing a sheet of mica with some phosphorescent powder and placing it edgeways along their path. In this way it can be shown that they travel definitely in straight lines, not colliding against each other, but each shot as it were like bullets from an immense number of parallel guns. Where they strike the sides of the glass they make it phosphoresce; where they strike residual air in the tube, as they do if the exhaustion is not high enough, they make it phosphoresce also, and give, in fact, the ordinary glow surrounding the dark space.

These rays possess a considerable amount of energy, as can be shown by concentrating them by means of a curved saucer-shaped cathode and bringing them, as it were, to a focus. A piece of platinum put at that focus will (if the exhaustion is not too high) show evident signs of being red-hot—that is to say, will emit light. If the exhaustion is higher less heat is produced, though a phosphorescent light is emitted from suitable substances like alumina and most earths; but if the exhaustion is pressed further still the bombarded target emits no visible light but that higher kind of radiation known as Röntgen or X-rays. It may be doubted, however, whether the target itself emits these rays, whether its function is not rather to stop the projectiles as suddenly as possible by the massiveness of its atoms. Thus the best target would be a substance with the heaviest atoms. The X-rays are probably emitted by the suddenly stopped projectiles in a manner which has been investigated both by Sir G. Stokes and Professor J. J. Thomson, and which is intelligible to anyone who has studied the properties of moving electric charges moving at the speed of light: a matter on which Mr. Heaviside has written with extreme clearness in his volume called *Electromagnetic Theory*.

Cathode rays have a remarkable penetrating power; for Hertz found that a thin metal diaphragm, especially if it were of aluminium, was powerless to stop their passage completely; as could be demonstrated by the phosphorescence and other effects appearing in the further half of the tube beyond the diaphragm.

The position of the anode in such experiments is of small consequence. There must be one somewhere, and the easiest plan is to make it a cylinder through which the cathode ray bombardment goes. The bombarding particles fly in straight lines and decline to turn a corner, taking no apparent notice of the position of the anode, and exhausting themselves by bombarding the side of the glass opposed to them if the tube is bent into a V shape, for instance.

Lenard extended Hertz's discovery in a remarkable way by skilfully constructing a tube with its outer wall of very thin aluminium, so arranged as to be able to stand the atmospheric pressure outside. He then directed the cathode ray bombardment on to this window or

aluminium film, and showed that the rays can penetrate it and actually come outside into the ordinary atmosphere, where they are called Lenard rays, in honour of this indefatigable investigator, a friend and disciple of Hertz.

These Lenard rays make the air phosphoresce and produce the other effects which cathode rays can produce, but they are stopped within a moderate range by the immense obstruction they meet with from a substance of the density of ordinary air. Substances seem to stop them simply in proportion to the quantity of matter which they encounter, without regard to its nature. A thick layer of air would be about as opaque as a layer of water $\frac{1}{800}$ as thick; and even if the body put in their way is a solid, provided it is thin enough and not too massive, it will be penetrated by the rays; and phosphorescent effects will be produced on the other side of it. The rays can also affect photographic plates, and indeed do nearly all the things, though on a smaller scale and with much less penetrating power, that the later discovered Röntgen rays can do.

The Lenard rays are clearly cathode rays emerged from the tube, and it was the custom, at the date of their discovery, to think of them as flying charged particles of matter; though the extraordinary distance they could travel through common air, a distance comparable to an inch, was a manifest difficulty to such a hypothesis, seeing that things as big as atoms of matter cannot travel so much as $\frac{1}{1000}$ of an inch in ordinary air without many collisions.

Lenard accordingly adhered to the view that they were not material but ethereal; and although in the sense he probably intended this is not a tenable view, for they are not ethereal waves or anything of the nature of radiation, yet, as we shall see, neither are they ordinary material particles, any more than the cathode rays are. But that is just what we are now considering, and we will return to them as observed by Crookes in 1879.

Nature of the Cathode Rays.

We have seen that the impact of the cathode rays, speaking in language appropriate to the assumption that they are charged particles, will result partly in heat, or vibration of the impacted particles; partly in light or phosphorescence, due to the quiver of electrically charged atoms, or rather the electrical charges on atoms, as in the ordinary process of radiation; and partly in X-rays: all of which effects are readily seen at different stages of vacuum in a Crookes' tube. The *momentum* of the flying particles shot off from the cathode can also be exhibited by putting into their path some form of vane or little windmill, which will then be driven mechanically, as the vanes of a radiometer are driven by the recoil of the molecules of the residual air from the warmer surface, a stress being thus set up between the vanes and their glass enclosure. In the electric vacuum tube experiment the stress seems to be between the cathode or gun and the vanes or target, and the propelling force would appear to be the force of electrical repulsion, the particles travelling down the grade of potential just as they travel

in ordinary electrolysis; but whereas in ordinary electrolysis they meet with constant encounters and therefore progress very slowly, in the high vacuum they can fly for several inches in a free path without encountering anything, and therefore without causing any disturbance, giving rise to no appearance but that of the dark space. Phenomena occur only where they strike.

This was the view taken by the whole world of the nature of cathode rays after Crookes' demonstration; it was supposed that they were flying atoms, and that they were flying with ordinary molecular speed, but with a long free path—much longer than would have been expected from ordinary gaseous theory. The extraordinary length of free path was somewhat difficult to reconcile with the doctrine that they were flying atoms obedient to the ordinary laws of gases; except that, being subject to electrical propulsion all in the same direction, their course was more regular, and their encounters therefore fewer, than if they had been moving at random. This same feature of regularity it is that confers momentum upon them; their motion does not constitute heat, and is not to be considered as temperature; they are moving like a wind, rather than with the irregular unorganised motion appropriate, and solely appropriate, to the terms "heat" and "temperature," and to the ordinary kinetic theory of gases. Crookes indeed hazarded the surmise, by one of those flashes of intuition which are sometimes vouchsafed to a discoverer but are often jeered at by orthodox science at the time, that he had obtained matter in "a fourth state," and also that he had got in his tube something equivalent to what was contemplated in the "corpuscular" theory of light. There is something to be said for even this last mode of statement, when the particles are moving quickly enough; but how true the first was—that the matter in the dark space was in a fourth state, neither solid nor liquid nor gaseous—how true that was we shall presently see.

Meanwhile let us summarise the evidence for the view that the cathode rays are at any rate charged particles of some kind in extremely rapid motion. That they are in motion must be granted from the facts of their bombardment—driving mills, heating platinum, and the like; and in order to show that they are charged, the most direct plan is to catch them in a hollow vessel connected with an electroscope, as Perrin did; but another plan is to show that they have the properties of an electric current. If they are charged while in motion they constitute a current on Maxwell's theory, and therefore should be able either to deflect a magnet or to be deflected by it; and here comes one of the most simple and important experiments in physics at the present time. A definite form of old experiments by Goldstein and many other vacuum tube observers was arranged by Crookes in 1879, when he made the track of the rays visibly luminous by passing a selection of them through a slit and letting them graze along the surface of a film of mica covered with phosphorescent powder, and when he then brought near them a common horseshoe magnet. When this is done the track of the rays is at once seen to be curved; showing that it is not a beam of light we are looking at, but a torrent of charged particles behaving like an electric current and deflected by a magnet.

It is really the very same phenomenon as can be observed with difficulty when a current flows through metals, which was discovered by E. H. Hall, and known as the Hall effect.

The fact that the particles are thrown off the cathode, being evidently vigorously repelled by it, is sufficient to suggest that they must be negatively charged; the direction of the curvature caused by a magnetic field enables us to verify at once that the flying particles are negatively charged, and no comparable rush of positive particles in the opposite direction or in any direction has been observed. In that respect evidently the magnetic curvature of cathode rays in gases differs from the magnetic curvature of a current in metals, viz., that whereas in metals it is sometimes the negative and sometimes the positive current which is acted upon, according to the nature of the metal, and is always small, in gases it is the negative alone that appears to be acted upon and the action is always large. It seems, therefore, that for some reason or other the negatively charged bodies in a vacuum tube are much more mobile than the positive, and that the mobility of the negatively charged bodies is extreme. One striking method by which their mobility was displayed consisted in the observation by Professor Schuster that all parts of gas in a closed vessel became conducting when an electric discharge had taken place in one corner of it, so that even though the vessel consisted of different compartments, one compartment was made feebly conducting by a discharge in the other, provided that the two had any kind of gaseous communication, a fact which looked as if some extremely mobile particles, probably the negatively charged particles of cathode rays, could wander about to a considerable distance in a very short time and take their share in the conveyance of an electric current. The conductivity of gases appeared to be, indeed, entirely due to these loose or dissociated or detached charged particles, and where they were absent the gas did not conduct at all; it could be broken down, being a weak dielectric, by a sufficiently strong force, but it would not leak; whereas, when these loose charged particles were about, it leaked readily, becoming to all intents and purposes an electrolyte amenable to the feeblest electric influence. And the act of breaking down the air by an electric discharge was found to render the surrounding air for a time thus electrolytic. Its electrolytic quality, however, did not last long. The mobility of the particles which enabled them to travel to a considerable distance also enabled them to get rid of themselves by clinging to the sides of the vessel, or perhaps by re-uniting to some opposite but comparatively immobile positive charges, which after some time in their rapid journeys they must casually encounter. Mr. Townsend,[1] however, found that the conducting power lasted unexpectedly long if no dust was present: the dust particles evidently acting as intermedial receivers and storers of charge, promoting interchanges, which otherwise might be delayed from accidental non-collision. And the time that thus elapsed before the whole of the

[1] Mr. Townsend of Trinity College, Dublin, then working in the Cavendish Laboratory, Cambridge, now Waynflete Professor of Physics in the University of Oxford.

conductivity disappeared from dust-free air suggested that the moving particles must be very small, so that collisions were comparatively infrequent.

The mobility or diffusiveness of a gas depends on its mean free path, and that depends on its atomic size; the smaller it is, the more readily can it escape collision. Hence it is the collisions are so rare in astronomy: the bodies are small compared with the spaces between them. The behaviour of charged particles seemed to indicate that they must in some cases be something smaller than atoms. It seemed hardly likely that material atoms could behave in the way they did, so it was recollected that it had occurred to some philosophers, among them Dr. Johnstone Stoney, that electric charges really existed on an atom in concentrated form, acting as satellites to it; so on that view it was just possible that these flying particles might be not charged atoms at all, but charges without the atoms, the concentrated charges detached, knocked off as it were in the violence of the discharge and afterwards going about free; travelling at an immense pace because they would still be liable to the full electric force that they had experienced before, and yet would have shaken off the encumbrance of the material atom with which they had been associated. It is true that no such disembodied charges or electric ghosts had ever been observed. All the experiments that had been made in electrostatics had been made on charged matter, the surface or boundary of the matter acting as the locality for an electric charge. The facts of electrolysis had suggested or proved that the atoms themselves could carry charges, and hence that if a liquid were electrified, what was really happening was that a number of the atoms on its surface turned their similarly charged poles outwards; and the same might, for all we knew, be true for metals also, and thus every charge seemed associated with matter.

Yet at the same time the occurrences at an electrode, where an ion gave up its charge and escaped without it, indicated the *possibility* that perhaps the electric charge could exist alone, at any rate that it could be handed from one atom to another, and thus might conceivably exist alone for an instant. During this momentary isolation some might, in the freedom of a rarefied gas discharge, possibly escape, and wander about free.

To such hypothetical isolated charges, the unit charge or charge of a monad atom, the name "*electron*" has been given, and when I speak of an "electron" I mean to signify the at present purely hypothetical isolated electric charge. Whereas by the term "ion" I always signify the atom and its charge together.

Now if the flying particles which constitute the cathode rays were electrons rather than ions, if they were detached charges, leaving the atoms behind them (probably leaving the atoms from which they were detached positively charged), their extreme mobility and diffusiveness and high speed would be perfectly natural: and although they would not be matter in the ordinary sense, yet no difficulty need be felt at their possessing some of the properties of matter, at any rate such properties as appertain to matter by reason of its having inertia,

because, as we have seen, an electric charge itself does possess a certain kind of imitation inertia. Hence these electrons in movement would possess momentum, and might therefore propel windmills; they would possess kinetic energy, and therefore might heat a piece of platinum; and if suddenly stopped by a massive target when travelling at a high speed they might readily give rise to phosphorescent appearances, and even to the sudden pulse of radiation known as X-rays. But the existence of this last property ought to be capable of clear deduction on electrical principles if the matter is further gone into.

Increase of Inertia Due to Very Rapid Motion.

But now arises the question whether the distribution of charge on a charged body, together with its lines of force, will remain constant and unaltered while the body is rapidly moving; because if the distribution of lines of force is altered, then perhaps the inertia due to their lateral motion may be altered too.

Thus, for instance, imagine that the lines of force of a body in motion became more concentrated towards the axis or line of motion; the effect would be at once to diminish the lateral component of their motion, therefore to diminish the magnetic force which that lateral component causes, and thus to diminish the apparent or electromagnetic inertia of the moving charge.

On the other hand, if the lines opened out and became concentrated towards the equator, or plane normal to the line of movement, then a greater component of their motion would be of a kind suitable to excite a magnetic field; moreover, since both the fields would by this concentration increase in intensity, the whole transmission of energy (V EH) would be greater, and the inertia would apparently increase.

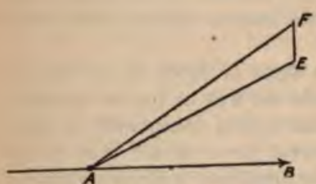
Thus, then, it may be possible that electric inertia may depend in some fashion on speed, a thing unknown in ordinary mechanics. I do not say that such dependence must be *untrue* in ordinary mechanics; on the contrary, I feel reasonably sanguine that it will be found true for matter moving sufficiently fast, and that it may even have a practical influence on some exceptionally rapid movements in astronomy. But however this may be, there is no doubt that theory points to an increase of electromagnetic inertia at excessively high speeds, and Mr. Heaviside has calculated its amount.

It will be observed that when a charge moves, it generates circular magnetic lines of force. Now these magnetic lines are not stationary, but are themselves moving at the same rate as the body, hence they generate fresh electrostatic lines, *i.e.*, cause an electric displacement away from the axis, which displacement is superposed upon the original radial displacement (away from or toward the centre) due to the charge.

At ordinary, at even violent speeds, this second order electric effect is insignificant, but it is there all the time, and must not be ignored when the speed becomes extravagantly high. It rapidly rises into prominence when the speed approaches the velocity of light, but at any speed much smaller than this such a second order effect is vanishingly small.

Its effect will be, as the above figure shows, to alter the distribution of the charge, making it move away from the poles and concentrate towards the equator of the charged sphere, when the speed is very great; ultimately becoming wholly concentrated upon the equator, all the rest of the sphere being denuded, when the speed attains that of light. And the electric lines of force will then be opened out into a fan or equatorial plane, like the spokes of a wheel which is rushing furiously along an elongated axle, the circumference of the wheel representing the direction of the magnetic field.

The magnetic force due to motion can be shown to depend on the ratio of the speed of the motion to the velocity of light, u/v. The secondary electrostatic force due to the motion of this magnetic field likewise depends on the same ratio. Hence the second order disturbance of the original uniform electrostatic field will be of the order u^2/v^2; and whenever we can afford to neglect quantities of this order, the distribution and therefore the inertia of the moving charge continue practically constant.



A is the charge, A B its line of motion, and A E its electric force in a certain direction when stationary; E F is the magnetically induced electric component due to the motion, and A F is the resultant electric force which replaces the original force A E. The magnetic force, to the motion of which E F is due, is perpendicular to the paper, and is itself caused by the motion; hence E F is a small quantity of the second order, for speeds distinctly less than that of light.

But when its speed of motion begins to approach the velocity of light, say even no more than $\frac{1}{10}$th of that speed, then a perceptible disturbance is to be expected, and something like a 1 per cent. increase of inertia must occur.

The complete investigation makes the inertia infinite when the speed reaches that of light, but there is probably no need to press this to extremes, unless the charge were an absolute point: clearly, however, the inertia will then be very great, and possibly therefore it may always be impossible to make matter, or at least charged matter, move with a speed greater than that of light. There may be ways out of this, however, just as it is possible for a bullet to move through air with a velocity greater than that of sound. This is managed by the violent adiabatic condensation of the air in front of such a bullet, the effect being to raise the appropriate velocity of sound to the required value.

If there is any way out of it in the case of the ether, however, it is not likely to be *this* way.

It has been shown both by Mr. Heaviside and by Prof. J. J. Thomson that if the speed of motion *is* ever greater than that of light, the fan or radial plane of lines of force bends backwards and becomes a conical surface, gradually closing up as the speed increases: an effect singularly reminiscent of the conical pulse travelling with a sufficiently rapid bullet, and demonstrated in Mr. Boys' bullet photographs.

No known speed which can be conferred upon matter is sufficient to

bring this latter effect into prominence. The quickest available carriage is the earth in its journey round the sun, 19 miles a second, or 60 times faster than a cannon-ball; but the earth's velocity is only the $\frac{1}{10000}$ of the speed of light, and consequently any spurious inertia due to its orbital motion is only 1 part in a hundred million; and even the accuracy of astronomy could not display any effect of that order of magnitude.

There are stars which move 200 miles a second, but even these have only one-tenth per cent. of the speed of light, and the excess inertia will be only 1 part in a million. The only known place where charges or charged matter move at speeds greater than this is in a vacuum tube. There the cathode-propelled particles are flying 20,000 miles a second or $\frac{1}{10}$th the speed of light, and they may have 1 per cent. excess inertia; or more if they can be persuaded to go still faster.

The substance of the above digression on the effect of rapid motion was written in connection with the Liverpool meeting of the British Association in 1896, and was communicated orally and very briefly to Section A in a discussion on the mechanism of the production of X-rays; for I then thought that unless great speeds, sufficient to disturb the static field, were reached by the cathode particles, they would not serve as efficient producers of the rays when suddenly stopped; but the matter has been gone into more fully now, and not only Mr. Heaviside's vol. 1 of *Electromagnetic Theory*, p. 57, may be referred to, where the circumstances of sudden stoppage of a charged body moving with the speed of light are illustrated, but also a paper by J. J. Thomson in the *Philosophical Magazine* for February 1898, dealing powerfully with the more general problem.

APPENDICES TO PART II.

APPENDIX C.

On Electricity and Gravitation.

Referring back to an article of mine in the *Philosophical Magazine* for November, 1882, page 358, we find the fundamental and necessary relation between constants stated thus, where M shall stand for magnetic pole and γ for Cavendish's gravitation constant—

$$e^2/K \equiv M^2/\mu \equiv \gamma\, m^2 = F\, l^2,$$

F being force and l being length.

If it is now going to turn out that a mass is composed of electric charges, it might seem as if e and m were quantities of the same nature, and were only numerically connected, whence it would follow that K and γ were of similar kind; in other words, Faraday's dielectric constant would become closely related to Cavendish's gravitation constant, and *weight* as well as *mass* would be traced to electricity; but such a deduction is unwarranted, there is nothing to prevent essentially

different properties of the ether being involved in the two kinds of force—gravitative and electric.

As to the nature of the gravitation constant itself we have—

$$\gamma = \frac{F\,l^2}{m^2} = \frac{l^3}{m\,t^2} = \frac{v^2}{m/l} = \frac{\text{sq. of velocity}}{\text{linear density}} = \frac{\text{energy/mass}}{\text{mass/length}}.$$

It is clear that if gravitation is in any sense of electric origin it must be a second order disturbance superposed upon the main electric effect, and be independent of sign. It would, in fact, depend upon e^4. For the gravitative force between two electrons at distance r would be—

$$F_1 = \gamma \frac{m^2}{r^2} = \frac{\gamma}{r^2}\left(\frac{2\mu e^2}{3a}\right)^2.$$

The electric force between the same two electrons at the same distance is—

$$F_2 = \frac{e^2}{K r^2}.$$

Therefore the ratio of the gravitative to the electric force at any distance is constant and equal to—

$$\frac{F_1}{F_2} = \frac{4\mu^2 K\gamma}{9a^2} e^2 = \frac{4\mu\gamma e^2}{9a^2 v^2} = \frac{2\gamma}{v^4} \cdot F_0$$

where F_0 is the electric force between electrons in contact, and v is the velocity of light.

Numerically this ratio of the two forces is—

$$\frac{F_1}{F_2} = K\gamma\left(\frac{m}{e}\right)^2 = \frac{1}{9 \times 10^{20} \times 1{\cdot}5 \times 10^7}\left(\frac{1}{10^7}\right)^2 = 10^{-42};$$

so the electric force exceeds the gravitative as much as the globe of the earth exceeds in bulk an ultra-microscopic object.

When there is an agglomeration of electrons of opposite sign their electric influence at a distance disappears, but their gravitative influences are simply added. So with 10^{21} mixed electrons in each of two bodies at any distance apart, the gravitative force between them will equal the electric force between two single electrons at the same distance.

In my 1885 Report to the British Association on Electrolysis, page 745, the following statement is made :—If the opposite electricities were extracted from a milligramme of water and given to two spheres one mile apart, those two spheres would attract each other with a force equal to the weight of 12 tons.

APPENDIX D.

Dimensions of e/m Ratio.

The reciprocal of the electrochemical equivalent of a substance e/m may be expressed as regards dimensions in several ways, one of which

exhibits it as a certain large numerical multiple of $\sqrt{(K\gamma)}$, the geometric mean beween Faraday's dielectric constant and Cavendish's gravitation constant. For hydrogen, this numerical multiple is of the order 10^{18}; for silver 10^{16}.

Another way is obtained by writing—

$$e^2 = K F l^2 = \frac{m l}{\mu},$$

whence it follows that—

$$\frac{m}{e} = \frac{\mu e}{l} = \sqrt{\left(\frac{\mu m}{l}\right)},$$

and so e/m can be expressed in $\sqrt{\left(\frac{\text{centimetres}}{\mu \text{ grammes}}\right)}$.

The artificiality of these dimensions is due to the fact that e and m have been conventionally measured in different ways; m is measured by ratio of applied external force to acceleration, while e is measured by repulsive force self-exerted on a similar charge at given distance.

If we express μ as a density (see "Modern Views of Electricity," Appendix *p*), the electrochemical equivalent comes out as expressible in grammes per square centimetre, that is to say a surface density.

It is noteworthy that while $\sqrt{(K\mu)}$ is of the same dimensions as $1/v$, $\sqrt{(K\gamma)}$ corresponds to $1/\epsilon$, where ϵ is an electrochemical equivalent.

PART III.

Determination of Speed and Electrochemical Equivalent of Cathode Rays.

The curvature of path produced in cathode rays by a transverse magnetic field, or the amount of rotation produced by a longitudinal magnetic field, constitutes an evident mode of attacking the problem of estimating their velocity.

If the velocity is constant and the magnetic field uniform, the curve into which the beam is bent will be a circle, and its course can be readily traced either directly, after Crookes' manner, by letting it graze a phosphorescent substance, or indirectly by inference from the position of a linear target placed so as to catch the deflected rays.

Consequently there will be no difficulty in determining the radius of curvature r; and the theory is the simplest possible, nothing more than stating that the magnetic force H, acting on the current element eu, is the necessary deflecting or centripetal force, mu^2/r, required to overcome the mechanical inertia of the particles; *i.e.*,

$$\frac{m u^2}{r} = \mu e u \mathrm{H},$$

$$\text{whence} \quad \left(\frac{m}{e}\right) u = \mu \mathrm{H} r;$$

or the ratio e/m is to the velocity of the particles as the curvature of their path is to the intensity of magnetic field which curves it.

The two factors on the right of this equation are directly measureable (μ being conventionally ignored as usual, or, what is a better mode of expressing it, measuring H as induction-density instead of as intensity of field), but the two factors on the left are both unknown, hence neither can be determined by this means alone : an assumption must be made about one or other of them, or else another independent kind of experiment must be made.

Assume, as many experimenters did, that u is a velocity appropriate to atoms flying in a gas of ordinary temperature, then the value of e/m comes out not so very far discrepant from the usual ionic value measured in liquid electrolysis, viz., 10^4 c.g.s. Or conversely, assume the usual ionic or electrolytic value for this ratio, and the cathode ray velocity comes out something quite appropriate to atoms of matter.

This, however, is a trap. These accidental coincidences may retard progress in a most serious manner, for they satisfy the mind and deter people from investigation. It is almost impossible to be completely on guard against them, and they are usually accepted until a more thorough qualitative acquaintance with the subject leads to an instinctive feeling that something is wrong somewhere.

So it was in this case, the long free path and the penetrating power of the cathode rays kept insisting that the particles were not really atoms of ordinary matter : a truth which both Lenard and Crookes had instinctively grasped, in spite of much criticism and valid arguments the other way; so in 1897 J. J. Thomson made a much more serious attack on the position.

He arranged that the magnet should deflect the rays into an insulated hollow vessel, connected with an electrometer and a known capacity, so that the aggregate charge of the cathode ray particles collected in a given time could be measured by the rise of potential observed. He also arranged that inside the hollow vessel they should fall upon a thermal junction of known heat capacity, connected by very thin wires to a galvanometer (acting therefore as a calorimeter), so as to measure their aggregate energy.

Thus he could make the following simultaneous determinations:—

$$N e = Q$$
$$N \tfrac{1}{2} m u^2 = W$$
$$\frac{m}{e} u = \mu H r$$

In these three equations there are four unknown quantities, but one pair can be treated as a ratio, and another, N, can be eliminated, and thus we get—

$$u = \frac{2W}{Q H r}$$

$$m/e = \frac{Q}{2W} \left(\mu H r \right)^2$$

When these brilliant measurements were actually made in the laboratory the atomic nature of cathode rays was, if not actually disproved, at all events rendered highly improbable; for their speed was found to be of the order ten thousand miles per second, or even as high as $\frac{1}{10}$ that of light in a favourable case, being always of the order 10^9 c.g.s., while the electrochemical equivalent was of the order 10^{-7} c.g.s., or about $\frac{1}{1000}$ that of hydrogen.

Changing the kind of residual gas in the tube, and changing the electrodes, made no difference to this last value. *The cathode rays were evidently independent of the nature of the matter present:* an exceedingly momentous fact. If they were matter at all, they appeared to be matter of some fundamental kind independent of the distinctions of ordinary chemistry. Their velocity, however, depended on the potential difference between the electrodes, in a way that suggested that they were really projectiles urged by the potential gradient acting along a given length of path. They were propelled by the cathode through an aperture in the anode, and the measurement of their speed was made in the tube beyond the anode, where they are travelling by their own momentum. The distance apart of anode and cathode did not, and on the projectile hypothesis ought not to, affect this speed; for though the potential gradient is steeper when anode and cathode are put close together, the length of path during which the particles are subject to it is diminished by a compensating amount, so that the velocity is theoretically independent of the distance between the electrodes, as long as the total difference of potential is maintained; it is the absolute difference of potential that determines the speed. But manifestly if the electrodes are too close together it may be difficult to secure a high difference of potential between anode and cathode, since they may spark into each other outside the tube; and if there is much residual gas in the tube it will likewise be difficult to maintain a high potential difference, because that residual gas, under the influence of the cathode rays, will conduct. Consequently the best speeds are obtained at high vacuum; and if the density of the residual gas inside the tube is constant, the speeds will be constant. The nature of the electrodes makes no difference, unless they give off gas or otherwise make it difficult to maintain the required potential difference.

Although the speed of the particles in cathode rays was thus found excessively great, their energy was only moderate, and their aggregate mass therefore excessively minute; their aggregate electric charge, however, was considerable. They were able to raise an electrical capacity of 1·5 microfarads several volts, sometimes as much as 20 volts, in the course of a second; and in the same time they might be able to raise a calorimeter, whose heat capacity was about 4 milligrams of water, by 2° C. Nevertheless their mass was so small that it would have taken one hundred years to collect a weighable amount, and then only about one-thirtieth part of a milligramme. They travelled with a velocity a hundred thousand times greater than the speed of rifle bullets, and represented the greatest velocity up to that time observed or even now known in matter, if matter they were; and the electrochemical *equivalent, instead of* coming out in accordance with that

observed in liquids, came out some thousand times smaller; that is to say, the charge associated with each particle of the cathode rays seemed a thousand times greater in proportion to the mass than the charge associated with an electrolytic ion, even of hydrogen.

If the flying particles were really atoms, there was no escape from the certainty that they were extraordinarily highly charged atoms; but if, as seemed more likely to the instinct of most of those who worked at the subject, the charge on the flying particles was the same as the charge possessed by an atom in electrolysis, then, assuming that the experiments were correct and correctly interpreted, there would be no escape from the conclusion that the mass associated with the ionic charge in cathode rays must be a thousand times smaller than the mass of a hydrogen atom; in which case the cathode projectiles might conceivably be the detached and hitherto hypothetical individual electrons or atoms of electricity themselves. It would be extremely rash, however, to jump to such a far-reaching conclusion on such comparatively scant evidence. The evidence must be confirmed by other departments of Physics or by other determinations based on a different method; and they must be further scrutinised in the light of the magnetised-radiation phenomenon observed by Professor Zeeman of Amsterdam. We will first describe a determination made by another method, and then some striking measurements applied to phenomena which belong apparently to other departments of Physics.

Further Measurements of Cathode Ray Velocity and m/e Ratio by Aid of Electrostatic Deflection.

Another and perhaps simpler method of determining the two quantities u and m/e was also employed by J. J. Thomson, viz., by deflecting the same rays both electrostatically and magnetically; by introducing a pair of supplementary electrodes, one above and one below the course of the rays inside the vacuum tube, and connecting them to the poles of a low potential battery, a few storage cells for instance, thus obtaining a vertical electrostatic field at right angles to the cathode rays. At the same time a magnetic field, produced by lateral magnet poles or by the lines of force due to an electric current in a circular ring, could be arranged at right angles to both the other directions; and thus the electrostatic deflection could be compared with, or used to neutralise, the magnetic deflection.

Let the cathode rays be received upon a needle-point covered with phosphorescent material and movable up and down in a measured manner; then the deflection of the rays can be observed by reading how much the needle has to be moved in order to catch a narrow beam which has travelled through a length l of either an electric field of strength E, or a magnetic field of strength H.

If u is the original velocity of the ray particles, travelling at right angles to both the deflecting fields, either of them will have a time l/u in which to act; and in that time an extra component w will be caused in the direction of the electric force, or perpendicular to the direction of the magnetic force, such that the rate of change of momentum of

each particle will be $\frac{m w}{l/u} = \mathrm{E}e$ in the one case, and $= \mu \mathrm{H} e u$ in the other; wherefore the deflection will be—

$$\theta = \frac{w}{u} = \frac{e}{m} \cdot \frac{\mathrm{E}l}{u^2} \text{ in the one case,}$$

$$\text{and } = \frac{e}{m} \frac{\mu \mathrm{H} l}{u} \text{ in the other.}$$

Hence if in the actual experiments the two kinds of deflection be made *equal*, by adjusting the relative proportion of the two fields, we get simply—

$$u = \frac{\mathrm{E}}{\mu \mathrm{H}}$$

$$\text{and } \frac{m}{e} = \frac{l \mu^2 \mathrm{H}^2}{\theta \mathrm{E}}.$$

This method, when applicable, appears to give fairly accurate results; and the outcome of the measurements is that when H or CO_2 or Air is in the tube—

$$u = 2 \text{ or } 3 \times 10^9 \text{ centimetres per second,}$$

$$\text{and } \frac{m}{e} = \text{from } 1{\cdot}1 \text{ to } 1{\cdot}5 \times 10^{-7} \text{ c.g.s. units.}$$

The chief difficulty about this mode of experimenting is caused by the fact that the ionisation of residual air in the tube causes it to become a temporary conductor, and so to screen the flying particles from most of the electrical influence. There is no guarantee that they feel the full effect of the electric field which is ostensibly being applied; indeed it is not easy to let them feel any of the effect. It used to be thought that they were not susceptible to electrostatic action at all, and this was often adduced as an obvious argument against their being electrically charged particles; but fortunately Thomson soon surmised the cause of this masking of the simple effect to be expected, and succeeded in showing that with high enough vacua and other precautions the screening ionised atmosphere could be removed, and the electrostatic deflection metrically observed.

Determination of Electrochemical Equivalent in the Case of Electric Leakage in Ultra Violet Light.

The same ratio of $m : e$, or a ratio of quite comparable magnitude, is obtained from phenomena which at first sight appear to be distinct.

One of these phenomena is the effect of ultra-violet light in discharging negative electricity from a clean metal or other surface; a phenomenon the investigation of which was begun by Hertz, and continued especially by Righi and by Elster and Geitel. (See one of the appendices to my "Signalling without Wires," published by the Electrician Co.) If ultra-violet light, whether from a spark or from a flame, fall upon a negatively electrified surface, then in general there will be a leak of electricity *from that* surface, which electricity can be received *by any body placed opposite* the illuminated one, and can be

used to charge an electrometer of known capacity, and so be measured. The writer has made very many experiments in this subject, which, however, have not yet been published. Now Elster and Geitel made the notable discovery that the introduction of a magnet affected the rate of leak, according to the direction of its lines of force. This phenomenon suggested a magnetic deflection of the lines of leak, which were shown by Righi to be singularly definite trajectories, and indicated that the leakage was due to the bodily propulsion of negatively electrified particles analogous to the cathode rays. A vacuum is not necessary to observe the effect, but in a vacuum the effect is more prominent and more accurately measurable. The difference between this case and an ordinary vacuum tube case is that there is no great E.M.F. or gradient of potential applied, there is accordingly nothing of the nature of a disruptive discharge; and in fact there is no leak at all until by the stimulus of the presumably synchronous vibrations of ultra-violet light the molecules are thrown into a state of agitation, and the attachment of the negative charge, or of some negatively charged corpuscles, thereby loosened.

Two things are necessary to get the particles away from the plate; they must be loosened by the impact of ultra-violet light—the direction of polarisation of this light having a very decided influence,—and the surface to which they cling must likewise be negatively charged, so as to repel them. Neither light alone nor electrification alone will produce the effect; co-operation is necessary.

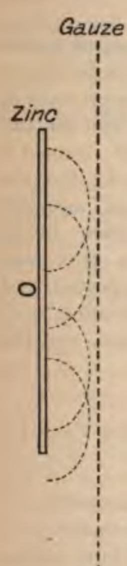


FIG. 1.

J. J. Thomson devised a most ingenious method of carrying out this experiment in a metrical manner, and of deducing from it the electrochemical equivalent of the charged particles, that is to say the amount of matter which each contained compared with the electric charge which each carried. To this end he employed the usual arrangement of a small negatively charged zinc plate on which ultra-violet light from a distant arc-lamp could shine, through quartz, and also through a parallel piece of wire gauze connected with an electrometer. The distance between the zinc plate and the metallic gauze was variable, and the experiment consisted in observing how much electricity reached the gauze from the negatively charged plate, under the influence of light, first without, and then with, a magnetic field of measured strength applied crossways to the region between them.

A little calculation of extreme beauty showed him that the paths of the flying particles under magnetic influence would be *cycloids*, whose generating circles contained the ratio m/e as well as the ratio E/H^2; that is to say their trajectory, if it could be observed, would involve the electrochemical equivalent required and likewise the ratio of the electric to the magnetic field applied, as well as the absolute strength of the magnetic field.

The calculation is so simple that it may be given here :—

Let the figure show the zinc and the gauze facing each other, close together, with a gradient of potential $(V-V')/d = E$ between them ; and let a magnetic field of induction density H be applied normal to the paper.

Then the motion of a charged particle detached and propelled from the origin into the region between the plates, provided that the plates are in vacuum so that there is no resisting medium to interfere, will be—

$$\begin{cases} m\ddot{x} = \mathrm{E}\,e - \mathrm{H}\,e\dot{y} \\ m\ddot{y} = \mathrm{H}\,e\dot{x} \end{cases}$$

the initial values of $x, y, \dot{x}$, and $\dot{y}$, being all zero.

The solution of these equations, under these initial conditions, is—

$$\begin{cases} x = a\,(1 - \cos b\,t) \\ y = a\,(b\,t - \sin b\,t) \end{cases}$$

$$\text{where} \quad a = \frac{\mathrm{E}}{\mathrm{H}^2}\frac{m}{e} \text{ and } b = \mathrm{H}\,.\,\frac{e}{m};$$

and from these we see that, whereas x is oscillatory in accordance with a versine, ranging from 0 to $2a$ and back, y is both oscillatory and progressive, completing its period in a time $\frac{2\pi}{b}$, and increasing in every such period by the amount $2\pi a$. In other words, the equations represent a *cycloid* traced by the rim of a circle of radius a rolling on the zinc plate.

There is no known way of actually observing this quite invisible and purely theoretical trajectory; but when it is perceived that in accordance with this theory all the particles moving between the plates will have similar paths, so far as they do not come near the edge of either plate—in which case they would not be propelled so far—it becomes plain that there should be a critical distance within which the gauze would receive and intercept *all* the particles, and beyond which not a single one would be able to reach it. In the figure the gauze is depicted as set just beyond the critical distance, so that it would receive no electricity, even though the ultra-violet light were fully shining; but so that if either its distance from the zinc were diminished, or the electric field strengthened, or the magnetic field weakened, the gauze would at once come within range and receive a plentiful supply of charge from the hypothetical cycloidally-flying particles. And the critical distance at which this would happen—a thing easily experimentally observed—would be independent of the brightness of the ultra-violet light, and would be merely the diameter of the generating circle; in other words, the critical distance between the plates, when effective transfer of charge occurred, should be $2a$, or $\frac{2m\mathrm{E}}{e\mathrm{H}^2}$; a quantity which by this ingenious means could be measured. Wherefore the ratio m/e for this case can be experimentally determined, if E and H are both known. The apparatus employed is shown in Fig. 2.

The sharpness of actual experimental observation of the critical distance was not found quite so great as this simple theory would

indicate, because of disturbing causes, one of which was the presence of some residual air, interfering with the perfectly free path of the moving bodies; nevertheless it was sharp enough for fair determination; and the result was again, in this case also, that the ratio e/m came out 10^7 c.g.s., or more exactly 7×10^6; corresponding closely with the values found by J. J. Thomson, confirmed subsequently both by Lenard and Kaufmann, for the cathode ray particles.

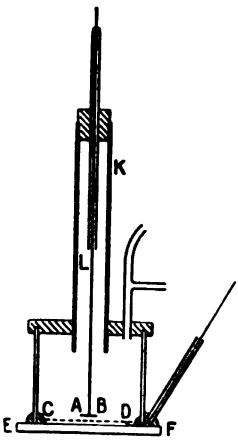


FIG. 2.—A B is the insulated zinc plate, C D is the gauze, E F is quartz; the source of ultra violet light is at some distance below, and the vessel can be filled with any gas and exhausted.

Another phenomenon on which measurements were made was the discharge of electricity from an incandescent carbon filament in an atmosphere of hydrogen. This also is subject to disturbance by a magnetic field, as was shown by Elster and Geitel; and a series of measurements, on lines similar to the preceding, resulted in a value—

$$\frac{e}{m} = 8{\cdot}7 \times 10^6 \text{ c.g.s.},$$

a value of the same order of magnitude as before, one thousand times greater than the electrochemical or electrolytic value for hydrogen, and many thousand times greater than for other substances, but always constant and independent of the nature of the substance present.

The only things which give the ordinary electrolytic value for this ratio are the *positive* carriers. These are not so easy to observe, but WIEN [1] has examined these by detecting and measuring the slight magnetic deflexion exhibited by certain rays behind the cathode in a vacuum tube, which Goldstein discovered and called *Kanal-strahlen*, and which Ewers proved were carriers of positive electricity. Wien has shown that they move slowly, and that in hydrogen their ratio e/m is of the order 10^4, that is to say the proper value for a hydrogen atom or ion; and with other substances the ratio has been found to vary with the substance and approximately to equal the electrolytic value, for these positively charged particles. J. J. Thomson has likewise made measurements on the positive carriers by means of the discharge from incandescent filaments and other positively charged hot bodies, and has confirmed Wien's results.

Thus it is forcibly suggested that whereas the positive carriers of electricity are *ions*, consisting of a unit + charge associated with an atom, the negative carriers appear to be dissociated from the main bulk of the atom, as if they were only fractions or fragments or constituents or appendages of an atom, which, detached and flying loose, are able to attain to prodigious speed; since any acceleration to which

[1] *Wied. Ann.* lxv. p. 440. See also Ewers in Wied. Ann. lxix. p. 187.

they are subjected is a thousand-fold greater than it is even for an atom of hydrogen, weighed down and burdened as that is with a mass of inert material and subject only to the very same propulsive force.

Think of the mobility of a set of particles which experienced the usual gravitation intensity g and had only $\frac{1}{1000}$ of the mass to carry. There is no known way of thus intensifying gravity—there are plenty of ways of diluting it, *e.g.* Atwood's machine, an inclined plane, etc., etc. But such a mobile particle as that we are now considering would drop under the influence of gravity not 16 feet in the first second, as everything we know does near the surface of the earth, but 16,000 feet, or about three miles; and would in one second acquire under gravity a velocity of six miles per second, enough almost to carry it out of the range of the earth's attraction altogether, and more than enough to carry it round the world.

The acceleration to which such particles are subject in a vacuum tube is far greater even than this, because there the forces are so prodigious; gravitation force on ions is almost infinitesimal compared with common electrical force on their charges. Suppose, for instance, that they are in a field such as easily occurs in a vacuum tube, of 3,000 volts per centimetre, one-tenth of what ordinary air will stand, or ten electrostatic units. The force urging one of these carriers to move is then $10 \times 10^{-10} = 10^{-9}$ dyne; the mass being moved, if it is a whole atom of hydrogen, *e.g.* if it were a positive carrier in a hydrogen atmosphere, is only 10^{-24} gramme, and accordingly the acceleration it experiences is 10^{15} centimetres per second per second, or a billion times g. Whereas if it were a negative carrier, in any atmosphere, its acceleration would be a thousand times greater still.

The velocity acquired in passing over a distance of five centimetres under this force is obtained by finding the square root of $2fh$; that is to say, it is 10^8 centimetres per second for a positive carrier, and 3×10^9 centimetres per second for a negative carrier; and these are approximately the orders of magnitude actually observed.

Thus the hypothesis becomes more and more justified that these units of electric charge can separately exist; perhaps carrying with them part of the atom, in which case they might be called corpuscles, having a material nucleus; perhaps pure disembodied electricity, whatever that may be—an electrical charge detached from matter,—in which case they would correspond with those hypothetical entities familiar in theoretical and mathematical treatment as "electrons."

APPENDIX TO PART III.

APPENDIX E.

ELECTRIC SATURATION, ETC.

In my Report on Electrolysis to the British Association for 1885 (see the Aberdeen volume, pp. 762, 763), I call attention to the possibility that an atomic *theory of electricity* would give rise to a maximum charge

possible on a given area. The maximum surface density would be attained when every atom was polarised so that its atomic charge faced outwards; and for a solid or liquid it would be very great. For the charge on each being 10^{-10} and the number of atoms per square centimetre being 10^{16}, it follows that the maximum surface density possible is $\sigma = 10^6$ electrostatic units per square centimetre. The corresponding gradient of potential would be $4\pi\sigma = 10^7$, or 3,000 megavolts per centimetre, and the corresponding tension would be $2\pi\sigma^2 = 6 \times 10^{12}$ c.g.s. = 40,000 tons to the square inch. Of course no dielectric would stand this pressure, but absolute vacuum might.

In practice, therefore, it follows that when a surface is charged highly, only an exceedingly small percentage of the molecules are polarised with their charges facing outwards. For instance, common air breaks down when the tension rises to a value $2\pi\sigma^2 = \frac{1}{2}$ gramme per square centimetre = 400 c.g.s.; wherefore the maximum σ in ordinary air is 8 electrostatic units per square centimetre; and this quantity would be afforded by the facing outwards of 10^{11} molecules, or one in every hundred thousand of a solid surface, or about a tenth per cent. of those in air.

It is shown on p. 760 of my 1885 B. A. Report on Electrolysis, that a potential gradient of the order 1 volt over molecular distance is sufficient to overcome atomic attraction and effect decomposition in liquids. Any liquid which is a conductor throws the whole applied stress on to a molecular layer contiguous to an electrode, and accordingly something of the order of a volt or two difference of potential between electrodes in such a liquid is required, and is sufficient, for decomposition.

PART IV.

THE ELECTRON THEORY OF CONDUCTION AND OF RADIATION.

Meanwhile the probability of the existence of electrons and the possibility of regarding them as the basis of all electric and of most other material phenomena, had seized hold of the imagination of several mathematical physicists, notably of Dr. J. Larmor and of Professor H. A. Lorentz. Both these philosophers endeavoured to trace all electric properties to the behaviour of electrons, usually of course in association with material atoms; and Dr. Larmor proceeded to try and invent a possible structure in the ether which should have the properties of an electron, whether positive or negative, and so reduce a great part of Physics to its simplest terms. This magnificent attempt at a new Principia has not yet been finally successful, but a great mass of very suggestive material is to be found in Dr. Larmor's contributions to the Transactions of the Royal Society, and in his recent great summary called "Ether and Matter"; which last, published by the Cambridge University Press as an Adams Prize Essay, is accessible, though barely intelligible, to all.

Suffice it here to say that the electron constitutes the basis of the

whole treatment, and that there is supposed to be no electric current except electrons in motion. They may move with the atoms, as in electrolysis; they may fly alone, as in gases; or they may be handed on from one fixed atom to the next, as in solids.

CONDUCTION.

The possible modes of conduction or transmission of electricity are in fact three, which I may call respectively the bird-seed method, the bullet method, and the fire-bucket method.

The bird-seed method is adopted in liquids: it is exemplified in electrolysis; the bird carries the seed with it, and only drops it when it reaches an electrode.

The bullet method is the method in gases, as has been clearly realised by aid of the cathode rays: the space from cathode to anode represents the length between the breech and the muzzle of the gun, and the rest of the path is analogous to the trajectory of a bullet, which ultimately either penetrates or is stopped by a target, with a flash of light or other appropriate disturbance.

The fire-bucket method must be the method of conduction in solids, where the atoms are not susceptible of locomotion and can only pass electrons on from hand to hand; oscillating a little in one direction to receive them, and in another direction to deliver them up, and so getting thrown gradually into the state of vibration which we call heat. But it may be observed that this need for motion, in order to pass electrons on, becomes less and less according as the body is less subject to the irregular molecular disturbance we call heat. It may be the expansion and molecular separation, or it may be the irregular jostling and disturbance, that impede easy conduction; but certainly conduction improves as temperature falls, and transmission becomes quite easy at very low temperatures. The conduction of heterogeneous alloys is a less simple matter, being probably mixed up with back E.M.F. developed at innumerable junctions,—otherwise it would be instructive to examine the effect of low temperature on the conductivity of a metal which did not contract with cold. The extra conductivity of hot electrolytes is a totally different phenomenon: it is not true conduction, but convective locomotion of ions in their case.

Metals are bodies in which the transfer of an electron from one atom to another is easy, demanding no force as long as the process is not hurried—a process of the nature of a *diffusion*; insulators are bodies in which it can only be accomplished with violence. The transmission of vibrations along a chain of connected molecules may well occur through a not dissimilar kind of connection; and hence the conduction of electricity and the conduction of heat, though really different processes, may have many points in common.

Most is known about electrolytic and gaseous conduction. In gaseous conduction the negative electrons fly free and fast; the positive charges travel slowly by reason of their association with matter.

In liquid conduction both sets of electrons are associated with

atoms, and travel only as ions at a slow diffusion rate which was calculated by Kohlrausch, has been observed by myself and others, and is well known.

The rate of transmission in solids can only be inferred, and it would appear as if in one class of solids the positive were able to travel fastest, whereas in another class negative travelled fastest: a difference which is familiar in liquids. In acids, for instance, the positive charges travel much the quickest, because they are associated with light hydrogen atoms; and it is owing to the comparatively easy migration of this light or small hydrogen atom that acids are in general such good conductors.

The Hall magnetic bend, like Faraday's magnetic rotation, is a differential effect, and would be zero if positive and negative were equally acted on. In gases it is differential too, but there the negative charges are so free as compared with the positive, and fly so much more rapidly, that the Hall effect in gases, especially in rarified gases, is very great in comparison with the small residual effects found in liquids and solids.

RADIATION.

But it is not only the progressive motion or locomotion of the electric atomic appendages that we have to consider; we must assume also that they are susceptible of motion in the atom itself, either vibrating like the bead of a kaleidophone, or revolving in a minute orbit like an atomic satellite. Indeed it is to the vibrations or revolutions of the electrons in an atom that its radiating power is due. Matter alone has no perceptible connection with the ether, a fact which is proved in my paper in the Philosophical Transactions for 1893 and 1897[1]; it is electric charge which gives it any connection, and even then it has no *viscous* connection—there is no connection that depends upon velocity, or is of the nature of friction,[2]—it is purely accelerative connection; it is only when the charge vibrates, and during its accelerative periods, that it is able to influence the ether and carve it into waves[3]—waves consisting probably of alternations of shear, with no motion of the ether as a whole, but only a to-and-fro quiver of its equal opposite constituents over some excessively small amplitude: a kind of motion which constitutes what we know as radiation. It is not the atom pulsating as a whole which disturbs the ether, but the pulsations or vibrations, or the startings and stoppings and revolutions, of its electric charge. But normal or centripetal acceleration, involving nothing more than change of direction, is just as effective as actual change of speed. If an electric charge is able to describe a small orbit four-hundred-billion times a second, it will emit the lowest kind of visible red light. If it vibrates faster it will emit light of higher refrangibility; and the particular kind of radiation emitted by the atom of any substance, when in a fairly free state, will depend on the orbital period of its electrons: every frequency of vibration corresponding to a definite line in the spectrum.

[1] Lodge, *Phil. Trans.*, vol. 184, pp. 727–804, and vol. 189, pp. 149–166.
[2] See especially *Phil. Trans.*, vol. 189, p. 164.
[3] *See Part I. of this* paper, p. 49, also Appendix G.

But, if this be so, radiation must be susceptible to magnetic influence, for a revolving electric charge constitutes a circular current, and if a magnetic field is started into existence with its lines threading that circuit, it must, while it is changing in intensity, cause the speed either to increase or to decrease, and so will either raise or lower the refrangibility. If, then, electrons are revolving in every direction and a magnetic field is excited, during the rise of the field the pace of some will be increased and of some decreased, and this increase or decrease will not stop until the magnetic field is destroyed again.

Hence it would appear that if a source of radiation is put into a magnetic field, and its lines examined with a spectroscope, they should be doubled, some being raised in refrangibility, others lowered; or if any are left unaltered the line might be tripled, or if the motion was of a more complicated character the line might conceivably be quadrupled or sextupled, or any other change produced according to the character and complexity of the motion. At any rate it would seem that the line must be affected somehow, even if it were only broadened. It happened, however, that when Dr. Larmor theoretically perceived this, and did the calculation for it in 1895 to see how much effect might be expected, he made the natural assumption not that an electron could move by itself on a comparatively stationary atom, as above described, but that the atom was itself pulsating or revolving or quivering in some way as a whole and carrying its charge with it. On this assumption, knowing what he did about the massiveness of an atom, he perceived that the effect would be too small to see; and inasmuch as Faraday had, with imperfect appliances many years ago, looked for some such effect—not then guided by theory, but simply with the object of trying all manner of experiments—and had failed to see anything, no fresh experimental attempt to examine the question was initiated; nor was the matter publicly referred to until, as hinted above, Zeeman of Amsterdam, in 1897, with a powerful Rowland grating and a strong electromagnet, skilfully observed a minute effect consisting in a broadening of the lines emitted by a sodium flame placed between its poles; and then at once Dr. Larmor wrote to me, saying that this must be the effect which he had expected but thought must be too small to see. On receiving the intimation I immediately, with a little trouble, repeated and verified the experiment,[1] and exhibited it at the Royal Society soirée in May that same year.

From this simple but important beginning the large subject of the influence of a magnetic field on the radiation from different substances has been laboriously worked at; not only by the original discoverer, but by Preston in Dublin, Michelson in America, and others; and a whole series of important facts has been made out. Every line has been been studied separately; some lines are quadrupled, some tripled, some sextupled, and so on, as said above. One mercury line is resolved into as many as eleven components. The effect is therefore *not* too small to see, though it needs excessively high power and perfect appliances to see it; and so it became evident that if radiation were

[1] See *Proc. Roy. Soc.*, vol. 60, pp. 466, 513, and vol. 61, p. 413, or Nature, vol. 56, p. 237; also *The Electrician*, vol. 38.

due to moving electrons, their motion could not be handicapped by having very much matter associated and moving with them. It became possible, indeed, by making a measurement of the amount of doubling undergone by the lines in a given field, to ascertain how much matter was associated with the revolving electric charge in any given case; in other words, to make a determination of the electro-chemical equivalent effective in radiation, *i.e.*, of the ratio m/e. Indeed, Professor Zeeman, with considerable skill, made a rough determination of this kind at a very early stage, when he only saw the effect as a slight broadening of the sodium lines; and came to the conclusion that the electrochemical equivalent was quite different from that appropriate to electrolysis, being some thousand times smaller. He found, in fact, that the ratio e/m had in this case also the notable value already suspected in connection with cathode rays, viz., the value 10^7 c.g.s.

More recent measurements have confirmed this estimate, and shown that the ratio of charge to matter in the Zeeman case is practically identical with the ratio of charge to matter in the cathode ray case; in other words, that whatever is flying in the cathode rays is vibrating in a source of radiation, and that if the cathode rays consist of moving electrons, radiation is due to vibrating or revolving electrons.

Even this, however, does not constitute a proof of the existence of masses so much less than atoms; it may be only a remarkable coincidence. Besides, it is possible that in all these cases the whole atom is, after all, moving, but that its electric charge is one thousand times bigger than what had previously been observed as the proper charge of an atom.

But this assumption, improbable even for the cathode rays, becomes still more unlikely in the case of radiation, where it is not at all unnatural that only a very small part of the atom should be moving, the great bulk of it being practically stationary. Besides, the more the details of the Zeeman effect are studied, the clearer it becomes that the electron theory attributed to it from the first by Professor H. A. Lorentz, as well as by Fitzgerald and Larmor in England, is complete and satisfactory.

One of the earliest publications in England, both of the fact and of its elementary theory, is that given by the present writer in two articles in the *Electrician* for February and March, 1897,[1] which are worth referring to as representing incipient ideas on the subject before the full significance was grasped. The high value of the e/m ratio, viz., $\frac{1}{2} \times 10^7$ c.g.s., or fifty million coulombs per gramme, instead of the moderate electrolytic value, is spoken of on page 643 as a difficulty; and a Fitzgerald suggestion amounting virtually to the beginnings of an electron theory of the Zeeman effect is hinted at. Likewise an extremely short way of expressing the theory of the motion is given by the writer, in the following form:—

Consider the resolved part of any orbital motion projected on to a plane normal to the applied magnetic field H, and let the angular

[1] See Lodge, *Electrician*, vol. 38, pp. 568 and 643.

velocity at any point with radius of curvature r be ω, then the field will exert a radial component—

$$\pm \mu e \mathrm{H} r \omega$$

which will represent an increment or decrement of centripetal force

$$d(m r \omega^2),$$

whence it follows, to a first approximation, that—

$$d\omega = \pm \frac{\mu e \mathrm{H}}{2m},$$

and the change of frequency caused by the magnetisation will therefore be—

$$\pm \frac{\mu e \mathrm{H}}{4 \pi m}.$$

The other component of the original orbit will manifestly be unchanged. This is far from being a complete and satisfactory theory, unless the projected motion happened to be circular; but it was a brief and early attempt.

On the Electron Theory of the Magnetisation of Light.

Among the early contributions that have been made to the theory of moving charges, few are more remarkable than those of Dr. Johnstone Stoney in connection with the process of radiation, long before there had been any experimental verification of the separate existence of these electrons, or of the fact that the emission of light from a substance is due to their motion. Dr. Stoney had treated them in an astronomical manner, in 1891, dealing with an electron moving round an atom as if it were a satellite moving round a planet, and had discussed the various perturbations to which they might be subject, and the effect of those perturbations on the spectrum of the light emitted.

One of the simplest kinds of perturbation is what is called a progression or recession of the apses, being a slow revolution of the orbit in its own plane. Such a motion was shown to be able to account for a doublet in the spectrum; for of the two component vibrations into which the motion can be analysed, one has been made more rapid and therefore its light raised in refrangibility, the other has been made slower and therefore lowered in refrangibility.

Another closely allied kind of perturbation, analogous to precession of the equinoxes in the case of the earth, would result in a line triplet in the spectrum. This precessional motion occurs in an orbit subject to any oblique pull or deflecting force. Instead of yielding directly to that pull, its effect is to make the axis describe a kind of cone, the kind of motion that one sees in an inclined spinning-top: the pull of gravity on a spinning-top does not make it topple over, but makes it precess. So also with a hoop or bicycle when not vertical: instead of tumbling, it turns round and round in a circuit as long as its motion continues; only falling when the motion ceases. Hence if the orbit of an electron were subjected to an oblique or *deflecting* force, the effect would be not to place *it directly in the desired* position perpendicular to a line of force,

but to cause it to precess; and this motion might be analysed into three, the acceleration and retardation of circular orbit above-mentioned, which would result in a doubling of the line, and a third component, viz. the one parallel to the axis, which would be unchanged, and would therefore represent a spectral line in its old position, the centre of the group of three. All this was clearly perceived by Larmor and Fitzgerald in connection with Dr. Zeeman's discovery, though they were anticipated by his great compatriot the eminent physicist, H. A. Lorentz; to whom the most complete publication of the theory is due, being in several respects anticipatory of the experimental results. For it may be observed that the light emitted by the oscillation components above spoken of will be all of one definite kind, due to vibrations in one definite direction and therefore polarised. The kind of polarisation would depend on the aspect from which it was seen. If seen at right angles to the axis of precession, all three lines should be plane polarised, the middle line at right angles to the other two. If, however, it be looked at along the axis of precession, then there should be no middle line, because the axial vibration would then be end on; and the two side lines should be circularly polarised.

Directly Zeeman had demonstrated the fact that a magnetic field applied to a source of light was able to act as a perceptible perturbing cause, Professor Lorentz was at once able to predict the whole of that which has been here stated about the tripling of the line seen sideways to the lines of force, and the doubling of the line seen endways, with all the polarisations as just stated; because the lines of magnetic force constitute the precessional axis. And all these effects were shortly afterwards seen by Zeeman and others, and are characteristic of the simplest circular orbit.

At first sight one might be inclined to suppose that the orbits would all face round and set themselves normal to the lines of force, like so many circular currents; but that is to forget the inertia of the travelling electron. It is manifest that since a revolving electron constitutes a circular current, its *tendency* will be to set itself with its plane normal to the lines of force; but since by hypothesis the revolving electron has inertia, the current will not so set itself, but will yield to the deflecting force in an indirect manner as a top does; or as the oblate spinning earth does—as explained by Newton in the Principia,—the axis of rotation having a conical motion round the lines of force, a motion which is called the precession of the equinoxes in the case of the earth, and the Zeeman effect in the case of a radiating atom.

This is an account of the chief part of the Zeeman effect, and may be regarded as the most fundamental kind of disturbance caused by a magnetic field on a source of radiation. But there may be other minor disturbances, just as in the case of the earth, whose axis is not only subject to precession, but also to nutation—a nodding movement superposed upon the main motion. It is also quite possible for the middle line, or for the two outer lines, or indeed for all three lines, to be doubled; thus giving rise not to the standard triplet, but to a quartet or a quintet or even a sextet,—appearances seen and photographed for some lines of some substances by Preston.

Even the two constituents of the double sodium line behave differently: one of the sodium lines, D_2, which had appeared only broadened to Zeeman at first, really becomes a sextet. The other sodium or D_1 line becomes a quartet; and a complete study of the behaviour under magnetism of all the lines and groups of lines given by different substances must result in a great extension of our knowledge in many directions; in fact it is hardly too much to say that the discovery of Zeeman, in the light of the theory of Lorentz, has doubled the power of spectrum analysis to throw light upon the processes of radiation and the properties of atoms, and has opened up a new department, as it were, of atomic astronomy, with atoms and electrons instead of planets and satellites.

APPENDICES TO PART IV.

APPENDIX F.

Size of Orbit of Radiating Electron.

Consider two electrons of opposite sign revolving round each other with luminous frequency n at any distance d; or better, consider a free negative electron revolving round a comparatively fixed equal positive charge attached to an atom, at distance d.

The force between them is $e^2/\mathrm{K}\,d^2$, so the acceleration is—

$$\frac{e^2}{\mathrm{K}\,d^2} \cdot \frac{3\,a}{2\,\mu\,e^2} = \frac{3\,a\,v^2}{2\,d^2}.$$

But the acceleration is also expressible as $4\,\pi^2\,n^2\,d$. Therefore—

$$d^3 = \frac{3\,a\,v^2}{8\,\pi^2\,n^2} = \frac{3\,a\,\lambda^2}{8\,\pi^2} = \frac{3 \times 10^{-13}}{80}\left(6 \times 10^{-5}\right)^2 = 10^{-23},$$

which is "Kepler's third law" for the case, and indicates that the distance at which luminous frequency is attainable is the atomic distance 10^{-8} centimetre; in other words, that the electron is roaming over the surface of the atom. If it got nearer to the centre of force than this it would have to revolve quicker; and such rapid oscillations may be excited among the internal paired electrons by shocks and collisions, or other perturbation.

The most important aspect of the above calculation is that it corresponds with the hypothesis that the whole of the mass of an electron is electric, and none of it material or unexplained; for it shows that a pure electron is able to revolve at distances of the molecular order with luminous frequency.[1] The square of the wave length emitted is proportionate to the cube of the radius vector; provided the plane of the orbit contains the centre of force. Otherwise there may be constrained motion of smaller amplitude, analogous to that of a conical pendulum.

[1] *See Lodge in The Electrician* for March 12th, 1897, vol. 38, p. 644.

APPENDIX G.

The Radiating Power of a Steadily Revolving Electron.

Consider an electron revolving as above (Appendix F) in an orbit of atomic dimensions d with luminous frequency n; and calculate its radiating power.

The fundamental expression for the amount of energy emitted per second as waves in the ether, by a moving charge e, was given by Larmor in *Phil. Mag.*, December, 1897, page 512, also in "Ether and Matter," page 227, namely—

$$\frac{2}{3}\frac{\mu e^2}{v}\dot{u}^2,$$

where $\dot{u}$ is acceleration, and where μe^2 may be taken as 10^{-40} gramme-centimetre, according to most recent measurements. But in a circular orbit of radius d the acceleration is—

$$\dot{u} = \left(2\pi n\right)^2 d = 40\left(5 \times 10^{14}\right)^2 10^{-8} = 10^{23} \text{ c.g.s.};$$

therefore the radiating power of a single electron, so moving, is—

$$\frac{2}{3}\frac{\mu e^2}{v} \cdot d^2\left(2\pi n\right)^4 = \frac{2 \times 10^{-40}}{9 \times 10^{10}} \times 10^{46} = 2 \times 10^{-5} \text{ ergs per second.}$$

But the total available energy possessed by the revolving electron of linear dimensions a is only—

$$\frac{\mu e^2}{3a}u^2 = \frac{\mu e^2}{3a}\left(2\pi n d\right)^2,$$

namely its kinetic energy (for of course it cannot radiate away or dissipate its electrostatic energy), and this amounts to—

$$\frac{10^{-40}}{3 \times 10^{-13}}\left(2\pi \times 5 \times 10^{14} \times 10^{-8}\right)^2 = 3 \times 10^{-13} \text{ ergs},$$

its velocity being 3×10^7 centimetres per second, or one-thousandth that of light. So if the electron were isolated from any supply of energy, and if it could maintain the pace, it would at this rate radiate away all its kinetic energy in 10^{-8} of a second, that is to say in three or four million revolutions. This may seem a rapid rate of cooling, but it is not surprising for an isolated atom at a red heat.

We may express the ratio of the radiating power of a single electron to its total luminous energy, by the fraction—

$$\frac{2a}{v}\left(\frac{\dot{u}}{u}\right)^2 = \frac{2a}{v}\left(2\pi n\right)^2 = 8\pi^2 n \frac{a}{\lambda} = 70 \text{ million per second.}$$

In any large assemblage of atoms the radiation is not free and unre-[strained], nor is it unmaintained, like this; but it must always be [cons]*iderable at anything* like luminous frequency, and it is proportional

to the fourth power of the frequency. At a frequency which emits a wave ten times as long as a luminous wave the radiating power of a revolving electron is only one ten-thousandth of that above calculated, but even so it is significant; it must be remembered, however, that all substances are actually engaged in radiating energy, although at ordinary temperature there is usually absorption enough to compensate the loss.

The high speed of a revolving electron suggests, says Larmor, that it is apt to fly away tangentially with this sort of velocity when joggled off by any means; consequently he might attribute the high velocity of cathode ray particles to this cause rather than to propulsion by a gradient of potential. But it seems to me more likely that the orbital velocity is utilised only by those electrons which are flung off spontaneously from certain substances possessing one variety of radio-activity; whereas from charged surfaces, possessing a real propulsive force, electrons may be ejected so as to reach a speed higher than that, approximating more closely to the speed of light, a value which the rapid increase of inertia at such speeds would ensure that they should never actually attain.

APPENDIX H.

Faraday's Prophetic Nomenclature.

Students of the life of Faraday will remember that when he discovered the rotation of the plane of polarisation by a magnetic field applied to dense bodies in which light travelled along the lines of force, —wresting the secret from nature by strong and pertinacious experimental research that would not be denied, though the time was as yet by no means ripe for comprehension of the fact when it was discovered —he labelled his discovery in a fit of enthusiasm, "The Magnetisation of Light and the Illumination of Magnetic Lines of Force:" a label which puzzled contemporaries for a long time.

It is difficult to see what meaning he can have attached to these phrases; and for many years afterwards they appeared unsuitable misnomers, indicating a foggy conception of his own discovery.

It is not likely that his state of mind was really at all clear on the subject, and probably he would at a later stage have been willing to plead guilty to a less than lucid mode of conceiving the phenomenon; which nevertheless always specially pleased him, though when it was reduced to a mere rotation of the plane of polarisation, it seemed to many mathematicians and physicists to have lost its unique and surprising interest. It must always be remembered, however, that interest was never lost by either Lord Kelvin or Clerk Maxwell, and that it was the chief fact which incited Maxwell, many years later, to begin developing his electro-magnetic theory of light.

But how do the titles strike us now? Do they not indicate some extraordinary unconscious insight, such as is frequently experienced by a great discoverer in the enthusiasm of discovery? Remember that the Hall effect, the Zeeman effect, the Aurora Borealis, and Faraday's rotation *are all closely connected*, by means of the electron theory.

In the cathode ray tube the flying electrons are deflected by a cross magnetic field; or if they fly along the lines they are twisted into a spiral path round them. In the Aurora Borealis this effect is carried out in the upper region of the air on a gigantic scale, and the earth's magnetic "lines of force are illuminated" by flying electrons from the sun entangled and guided by them. In the Hall effect this same influence is felt by the slowly moving crowd of electrons as they are handed on from one atom to the next, causing a curvature of the current path, in which either positive or negative may predominate. In the Zeeman effect the same cause operates on the revolving and vibrating electrons associated with a radiating atom and constituting a source of light; wherefore we may truly say that the "light is magnetised," for the source of light is magnetised directly, and the effect is impressed on and retained by the light emitted, and is made visible by spectrum analysis.

The first intimation of that magnetic influence on light which lies at the base of all these at first sight apparently diverse phenomena was detected by Faraday in his slight differential rotation of the plane of polarisation in one direction or the other by a magnet, according as the positive or the negative electrons in the dense substance were most affected.

Hence the title which he affixed to his discovery: "The illumination of the lines of magnetic force and the magnetisation of light," may be regarded as a prophetic flash of genius.

A not altogether dissimilar flash has already been referred to, when Crookes hinted prematurely that in the cathode rays we had something like corpuscular light, and also like matter in a fourth state, neither solid, liquid, nor gaseous. For, whether quite right or not, he was far more right than the critics of those days who presumed to deride him.

PART V.

Determination of the Mass of an Electron.

So far, all the measurements quoted have resulted in a consensus of certainty respecting our knowledge of e/m for gaseous conduction and radiation; and the measurements made on the cathode rays in a Crookes's tube, or near a plate leaking in ultra-violet light, have likewise given us a knowledge of their velocity, and shown that it is about one-thirtieth of the velocity of light, more or less according to circumstances. But so far no direct estimate has been made of either e or m separately. The difficulty of making these measurements is great, because we are dealing with an aggregate of an enormous and unknown number of these bodies. It would not be difficult to make a determination of the aggregate mass of a set of projectiles, say N m, where N is the number falling on a target in a given time, by means of the heat which the blow generates; or better, perhaps, by the momentum which they would impart to a moving arm after the fashion of a ballistic pendulum; *provided* their velocity u were known as in this case it is. The

aggregate energy, $\frac{1}{2}$ N $m u^2$, or the aggregate momentum, N $m u$, could thus be found; but how is m to be separated from N?

Again, if the particles are collected in a hollow vessel attached to an electrometer of known capacity, it is not difficult to estimate the total quantity of electricity which enters the vessel in a given time, that is to say, to determine N e; but, again, how are we to discriminate e from N?

We may consider the following quantities experimentally determined, by researches carried on at the Cavendish laboratory and elsewhere and so far already described or indicated:—

$$e/m$$
$$u$$
$$\text{N}\,e$$
$$\text{N}\,m$$

See above, Part III., for measurements of these quantities for the case of cathode rays.[1]

Another thing that is comparatively easy to determine, especially in such cases as leak from a negative surface under the action of ultra-violet light, or the conductivity of air induced by the impact of Röntgen rays, is the total current transmitted; viz. the quantity N $e u$ the quantity of electricity conveyed per second. Measurements of this quantity have been made not only by Lenard[2] and Righi[3] and Thomson,[4] but in various gases by Rutherford,[5] now Professor at Montreal; by Beattie[6] and de Smolan at Glasgow, by Zeleny[7] of Minnesota, by McClelland[8] on hot gases from flames, and by McLennan[9] of Toronto.

Professor Zeleny in particular measured the velocity by a safe and direct method of making the particles fly against a wind down a tube, and observing the rate of the current of air which was just able to withstand their progress: these measurements constituting a satisfactory confirmation of Thomson's and Rutherford's more indirectly inferred results.

If only it were now possible to *count* the corpuscles or electrons, to determine the number N which are started into existence, or which enter the hollow vessel, or which take part in conveying the current in the case of a leak by ultra-violet light, we should no longer have to *guess* at the actual value of e and of m separately, but should have really *determined* them.

This brilliant research has actually been carried out by Professor J. J. Thomson, by means of a method partly due to Mr. C. T. R. Wilson, supplementing a fact discovered by Mr. Aitken, and interpreted in the light of a hydrodynamic theorem arrived at long ago by Sir George Stokes.

I must be excused for waxing somewhat enthusiastic over this matter: it seems to me one of the most brilliant things that has

[1] J. J. Thomson, *Phil. Mag.*, October, 1897.
[2] *Wied. Ann.*, vol. 63, p. 253.
[3] *Rend. della R. Accad. dei Lincei*, May, 1896.
[4] *Phil. Mag.*, November, 1896. [5] *Ibid.*, November, 1896, and April, 1897.
[6] *Ibid*, June, 1897. [7] *Ibid.*, July 1898. [8] Ibid., July, 1898.
[9] *Phil. Trans.*, vol. 195, p. 49, 1899.

recently been done in experimental physics. Indeed I should not take much urging to cancel the "recently" from this sentence; save that it is never safe for a contemporary to usurp the function of a future historian of science, who can regard matters from a proper perspective.

The matter is rather long to explain from the beginning, and I must take it in sections.

Aitken and Cloud Nuclei.

First of all, Mr. John Aitken,[1] of Edinburgh, discovered in 1880 that cloud or mist globules could not form without solid nuclei, so that in perfectly clear and dust-free air aqueous vapour did not condense, and mist did not form. (See, for instance, my lecture to the British Association at Montreal, in 1884, on "Dust"—*Nature*, vol. 31, p. 268.)

Without solid surfaces, in clear space, vapours could become supersaturated; but the introduction of a nucleus would immediately start condensation, and according to the number of nuclei, or condensation centres, so will be the number of cloud globules formed.

Every cloud or mist globule is essentially a minute raindrop, not floating in the least, but falling through the air—falling slowly because it is of such insignificant weight and is moving in a resisting medium—but falling always relatively to the air. A cloud may readily be carried up by a current of air, but that is only because the air is moving up faster than the drops are trickling down through it. No motion of the air disturbs the relative falling motion: the absolute motion with reference to the earth's surface is the resultant of the two.

The fact that nuclei are required for mist precipitation can be proved by filtering them out with cotton wool, and finding that as the nuclei get fewer the mist condensation differs in character, becoming ultimately what is called a Scotch mist, such as forms in fairly clean air; where since the dust particles are comparatively few, the centres of condensation are few also, and accordingly each has to condense a considerable amount, so that the drops are bigger, and not nearly so close together; wherefore they fall quicker like very fine rain. In perfectly clean elaborately-filtered air the dew point may be passed without any vapour condensing, and the space will remain quite transparent in spite of its being supersaturated with vapour.

The reason for this effect of, and necessity for, nuclei, is well-known in the light of Lord Kelvin's theory concerning the effect of curvature on surface tension,[2] because the more a liquid surface is curved the more it tends to evaporate, and an infinitely convex surface would immediately flash off into vapour. Consequently an infinitesimal globule of liquid cannot exist; vapour can only condense on a surface of finite curvature, such as is afforded by a dust particle or other body consisting of a large aggregate of atoms. For it must be remembered that a single grain of lycopodium powder contains about a trillion atoms, and a dust particle big enough to condense vapour need not consist of

[1] *Trans. R.S. Edin.*, 1880.

[2] *See, for instance*, Maxwell's *Theory of Heat*, 1891 edition, p. 290.

more than a billion, or perhaps not more than a million, atoms, and need by no means be big enough to be visible. It is, however, material enough to be stopped by a properly packed cotton-wool filter.

J. J. Thomson and Electrical Nuclei.

In 1888 it was shown by J. J. Thomson, in his book *Applications of Dynamics to Physics and Chemistry*, p. 164, that electrification of a body would partially neutralise the effect of curvature, and so assist the condensation of vapour on a convex surface.

Consider a drop of liquid, or a soap bubble; the effect of the convexity of the surface is to give a radial component of surface tension inwards, causing an increased pressure internally. The effect of electrification is just the opposite: it causes a direct pressure outwards, which goes by the name of the electric tension.

The way these depend on size is as follows:—

The radial pressure component of the surface tension T is

$$\frac{2\,T}{r} \text{ inwards.}$$

The electric pressure or tension is

$$2\,\pi\,K\,\sigma^2 = \frac{e^2}{8\,\pi\,K\,r^4} \text{ outwards.}$$

They are differently affected, therefore, by the size of the globule; hence at some size or other they must balance, and such an electrified convex surface will behave as if it were unelectrified but flat. Accordingly vapour which would refuse to condense on an ordinary convex surface, until far below the dew point, will begin to condense on it, if sufficiently electrified, the instant the dew point is reached.

The critical size at which the ionic charge enables a sphere of water to act as regards condensation as if it were flat, can be reckoned by equating the pressure to the tension, thus:—

$$\frac{2\,T}{r} = \frac{e^2}{8\,\pi\,K\,r^4}$$

$$\text{or} \quad r^3 = \frac{e^2}{16\,\pi\,K\,T} = \frac{10^{-21}}{50 \times 80} = \frac{1}{4} \times 10^{-24} \text{ c.c.}$$

whence $r = 10^{-8}$ approximately, or is of atomic magnitude.

Hence *ions* can condense vapour; and anything smaller which possesses the same charge can condense it still more easily.

In moist air, therefore, it would appear (parenthetically) as if electrons could hardly exist isolated, but must be associated with at least an atomic mass of matter.

Accordingly an electric charge assists vapour to condense; and a sufficient electric charge might cause it to condense on quite a small body—as *small even as an atom, or* smaller. Hence in the presence

of electrified ions or electrons, dust particles are not necessary for condensation. Vapour may condense on these electrical nuclei without the need for solids of finite curvature. The electrical nuclei cannot be filtered out by cotton wool : they will exist or can be produced in dust-free air. No doubt if they are passed through a great amount of metal gauze they may be diminished in number, but they are not easy to get rid of except by their own diffusion, which does ultimately enable them to pair off or to migrate to the sides of the vessel. They can be got rid of, most easily, however, by electrolysing the air, that is to say by supplying electrodes maintained at a few volts difference of potential. They will then immediately make a procession, as in electrolysis, only with much greater speed, because their motion is much less resisted or interfered with by chance collisions; so they will soon reach and cling to their respective electrodes, and in that case again no true mist can form.

While ions or electrons are present in considerable numbers a thick mist will form whenever the space is saturated with vapour, but it will be a mist of different appearance from the slight rain-like condensation which may be seen forming round the few residual dust particles. The mist globules will usually be of uniform size, and some estimate of that size can be roughly attempted by the diffraction colours which can be seen if a point of light is looked at through the mist: not, however, a very easy plan of making a trustworthy estimate.[1]

Electrical nuclei can be produced in various ways—by anything, in fact, which dissociates the air or which fills it with ions. Some are produced by the splashing and spray of water, some are given off from flames, and from red-hot bodies, they are produced in considerable numbers when Röntgen rays travel through air, they can be given off by radio-active substances like uranium, and they are easily emitted by a negatively charged metallic surface exposed to ultra-violet light.

Wilson and Metrical Cloud Condensation.

Mr. C. T. R. Wilson,[2] in his study at the Cavendish Laboratory of cloud formation under the influence of Röntgen rays and by other methods, devised a plan for precipitating a definite and known quantity of aqueous vapour in a visible form. This was done by an arrangement for making a sudden or adiabatic expansion of saturated air, and making it to a carefully measured amount. The apparatus employed is shown in Figure 3.

One test-tube moving inside another is employed as a piston, and by a certain arrangement the piston was enabled to drop with great suddenness and thus to produce a measured small exhaustion in the reservoir containing the gas under experiment ; saturated as it is with vapour, and supplied with electric nuclei. The mist at once formed, and the drops began to fall slowly, as usual. Mr. Wilson tried to get an estimate of their size from the colours, but it was difficult and unsatisfactory. If the size had been known, their number would have

[1] See C. T. R. Wilson, *Phil. Trans.*, 1897, A, vol. 189, p. 283.

[2] *Phil. Trans.*, A, 1897, vol. 189, p. 265.

been known too, because the measured amount of expansion had produced a known fall of temperature below the dew point, and so had condensed a known amount of aqueous vapour, which would be distributed equally among all the equal globules.

It occurred to J. J. Thomson that a better estimate of size could be made by observing their rate of falling, which is a thing not difficult to observe since they all fall together, being all of the same size. In any mist formed in a bell-jar it is easy to watch it settling down, by watching its fairly definite upper surface, a clear space being left above it which gradually increases in thickness as the cloud falls. The

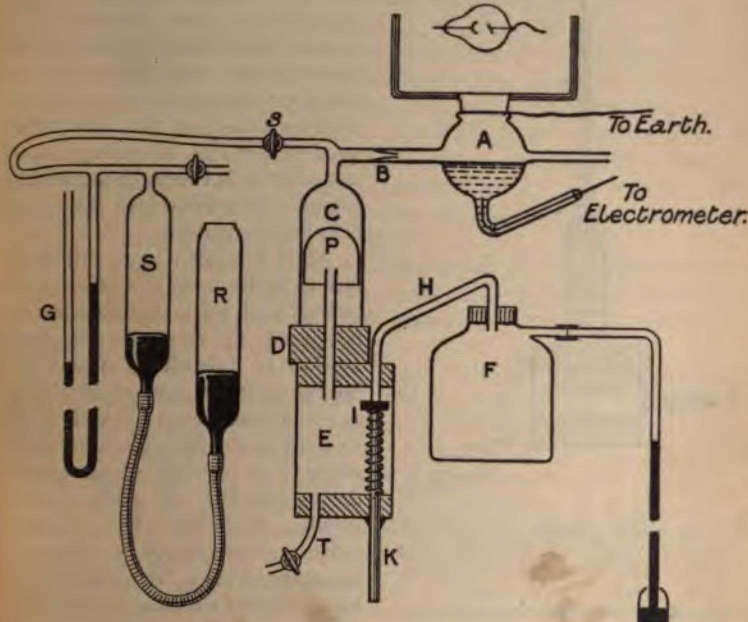


FIG. 3.—A is the vessel in which the fog is formed whose rate of fall is to be measured by Mr. Wilson's method as used by him for the ionisation produced by X-rays. The vessel A, containing some water, is in communication with a vessel C through the tube B. Inside C is a thin-walled test-tube P, which serves as a piston. D is an indiarubber stopper closing the end of tube C. A glass tube connects the inside of the test-tube P with a space E. This space may be put in connection with an exhausted space F through the tube H. The end of the tube H, inside the space E, is ground flat, and is closed by an indiarubber stopper I, which is kept pressed against the tube H by means of a spiral spring. The stopper I is fixed to a rod K; by pulling the rod down smartly the pressure inside the test-tube is lowered, and the piston P falls rapidly until it strikes against the indiarubber stopper D. The falling of the piston causes the gas in A to expand: the tubes R and S are for the purpose of regulating the initial pressure. Before an expansion the Piston P is raised by a trifling amount of air introduced through T, and the clip S is closed. Then, when everything is ready, K is *pulled, and the cloud* forms in A.

rate of movement of the top of the cloud will give the rate of falling of the individual globules of which it is composed. And this brings us to the next section.

Prof. Stokes and Falling Spheres.

Many years ago, in 1849, Sir George G. Stokes[1] discussed the motion of solids through fluids, and among others of a sphere moving through a viscous fluid urged by its own weight. It is a familiar fact that large bodies fall through air or water or any resisting medium more quickly than small ones of the same shape. Thus coarse sand settles down through water quicker than fine sand, and the finest powder takes a very long time to settle; in fact this difference of the rate of falling is used as a practical process of separating granular materials into sizes, and is called levigation.

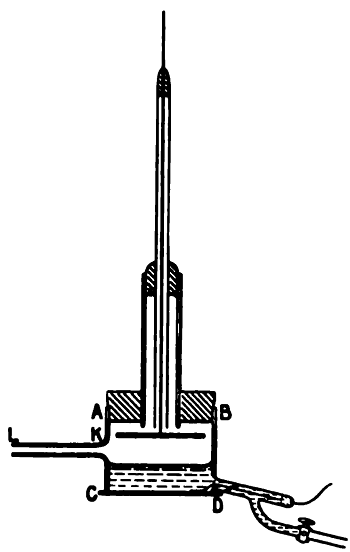


FIG. 4.—This figure corresponds closely with Fig. 2, Part III., only that a layer of water replaces the wire gauze. The vessel was attached to the expansion apparatus Fig. 3.

So it is in air: large raindrops fall violently, small raindrops fall gently, and mist globules hardly fall at all—fall so slowly that their motion is difficult to observe,—but the same law governs all so long as the motion is not too violent, or so long as the falling body has no edges such as will cause eddies during the fall. A sphere falling slowly, controlled by viscosity alone without waves or eddies, is the simplest case. It soon reaches what is called a terminal velocity—the speed at which the viscous resistance exactly balances its weight. At this speed it is subject to zero resultant force, so it simply obeys the first law of motion and moves at a constant speed.[2] This constant speed or terminal velocity was calculated by Sir George Stokes for the case of a falling raindrop of radius r as follows:—

$$c = \frac{2}{9}\frac{g\rho r^2}{\text{viscosity of air}},$$

where ρ is the excess density of the sphere over the medium it moves in; provided there is no finite slip at the surface. The maximum possible effect of surface slip—which will occur to some extent when the falling globules are very minute—is to make the possible terminal

[1] *Camb. Trans. Phil. Soc.* ix. 48.
[2] Cf. *Nature*, vol. 31, p. 266.

velocity half as great again: in other words, to convert the numerical coefficient $\frac{2}{9}$ into $\frac{1}{3}$.

This simple formula gives the connection between the rate of fall of a raindrop and its size; and by observation of this speed, therefore, knowing the viscosity of air, it is possible to calculate the dimensions of the falling drops.

J. J. Thomson's Experiment of Counting.

We have now all the materials ready for understanding the experiment to be performed,[1] so as to count the ions which are produced in air under the impact of Röntgen rays, or when there are electrons to be counted which have been produced from a negatively electrified surface illuminated with ultra-violet light. The apparatus for the latter is depicted in Figure 4. A clean zinc surface in vacuo, faced by a piece of wire gauze through which the light could shine on it, and by a window of quartz which makes the vessel airtight, so that it might be exhausted and yet allow the ultra-violet light to pass, was employed as shown in Fig. 2 above: the present arrangement is similar except that a water surface replaces the gauze.

The rate of leak which gives the current Neu is determined by connecting the water and the zinc plate to the terminals of an electrometer; the zinc plate being kept negative by means of a battery of a sufficient number of cells.

And now, supplying this apparatus with the adiabatic expansion appliances of C. T. R. Wilson shown in Fig. 3 above, metrical condensation can be produced, a mist will form, and the rate of its fall can be observed; whence by Stokes's theorem the size of each globule is known; the quantity of water which had gone to form globules is known from the measured amount of expansion, by a process the details of which I will not give here; and so the number of such globules, and therefore the number of their condensation-centres or nuclei or ions, can be determined.

If c is the observed rate of fall in stagnant air, the linear dimensions of the falling drops will be

$$r = \sqrt{\left(\frac{9\,\mu\,c}{2\,g\,\rho}\right)} = \sqrt{\left(\frac{4{\cdot}5 \times {\cdot}00018}{981}\,c\right)} \text{ centimetres.}$$

In a given case c was observed to be 0·14 centimetres per second; hence the volume of each drop was in that case

$$\frac{4}{3}\,\pi\,r^3 = 1{\cdot}6 \times 10^{-10} \text{ c.c.};$$

and so if the aggregate amount of water in all the drops in a given space is reckoned from the measured amount of adiabatic expansion which caused the chill and the precipitation, the drops can be counted.

A great many precautions must be taken, because there will be

[1] *Phil. Mag.*, December, 1898, and December, 1899.

where x is the "mean free path," or average distance travelled by any one particle without a collision with another, and a the number of encounters while travelling unit distance. But in saying this we are ignoring the forces between the particles, and are not considering a swing round as a collision.

So, as regards order of magnitude—

$$x = \frac{A x}{n \pi a^2} = \frac{1}{n_1 \pi a^2} = \frac{d^3}{\pi a^2},$$

with a factor $\frac{3}{4}$ or $\sqrt{2}$ omitted which a completer theory would give; where d^3 is the cubic space allotted to each particle, while $\frac{4}{3}\pi\left(\frac{a}{2}\right)^3$ is the actual bulk of each.

Therefore $\dfrac{x}{a} = \dfrac{\text{total space occupied}}{\text{eight times the aggregate volume of the particles}}$,

a statement sometimes quoted as Loschmidt's theorem.

Hence the mean free path can be determined by considering how much space the substance of all the electrons in an atom occupies, as compared with all the space which the atom occupies itself. In other words, we have to consider what the size 10^{-13} for an electron's diameter means, as compared with the size 10^{-8} for an atom's diameter. In the solar system the diameter of the earth is $\frac{1}{10000}$th part of the diameter of its orbit round the sun. Consequently if the earth represented an electron, an atom would occupy a sphere with the sun as centre and five times the distance of the earth as radius.

In other words, if an average atom is composed of electrons, they are about as far apart in that atom in proportion to their size as the planets in the solar system are in proportion to their size.

In an atom of hydrogen there are roughly 1,000, or say more exactly 700 electrons in order to make up the proper mass.

In an atom of sodium, which is twenty-three times as heavy, there must be about 15,000 electrons.

And in an atom of mercury there must be over 100,000 electrons.

Consider then an atom of mercury containing 100,000 of these bodies packed in a sphere 10^{-8} centimetre in diameter. One would think at first they must be crowded; but there is plenty of room. Each electron is only 10^{-13} centimetre across, and there are only about fifty of them in a row along any diameter of the atom; hence the empty space inside the atom is enormously greater than the filled spaces. At least a thousand times greater in linear dimension, or a thousand million times greater in bulk.

The whole volume of the atom is 10^{-24} c.c., the aggregate volume of all the electrons composing the atom is $10^5 \times 10^{-39} = 10^{-34}$ c.c., consequently the space left empty is 10^{10} or ten thousand million times the filled space.

Even inside an atom of mercury, therefore, the amount of crowding is fairly analogous to that of the planets in the solar system. For though the outer planets are spaced further apart than the inner ones, *they are also bigger*, to practically a compensating extent.

Now, going back to what is sometimes called Loschmidt's theorem in the kinetic theory of gases, obtained roughly above—

$$\frac{\text{mean free path}}{\frac{1}{6}\text{ diameter of particle}} = \frac{\text{volume of space available to particles}}{\text{combined volume of all their substance}}$$

we have reckoned the latter fraction, in the inside of an atom of mercury, as—

$$\frac{\frac{4}{3}\pi \times \left(10^{-8}\right)^3}{100{,}000 \times \frac{4}{3}\pi\left(10^{-13}\right)^3} = \frac{10^{-24}}{10^5 \times 10^{-39}} = 10^{10}.$$

Hence the mean free path of an *electron* inside an atom of mercury will be comparable to 10^9 times the size of an electron, *i.e.*, it will be 10^{-4} centimetre; that is, it may get through on the average the substance of some 10,000 mercury atoms in a row without collision.

In any other less dense substance it will go further. The actual *distance* thus travelled by corpuscles plunging into a dense metal is very small, only the thousandth part of a millimetre on the average, and it need by no means necessarily be a straight line; so a target of platinum succeeds in stopping them fairly near its surface, and enables the X-rays generated by the shock fairly to emerge. Some corpuscles will be stopped more suddenly than this, and some will travel further, but 10^{-4} centimetre is about the average distance travelled in a solid as dense as platinum.

This distance, however, gives no notion of the value of the negative acceleration during a collision, because the greater part of the thousandth of a millimetre is free flight; the stoppage occurs only as the last episode of that flight, viz. at the instant of collision. The colliding masses are 100,000 to 1, so the change of velocity at impact could be estimated; but the impact will really be more of an astronomical or cometary character, and the effect is analogous to the entrapping of comets when they pass near a planet, thereby rendering them permanent members of the solar system. *

The *ordinary* behaviour of a foreign comet, which comes and goes, may be called a collision with, and rebound from, the sun; for although there is no real encounter of main substance, that is what it would appear like if it could be seen from the depths of space; and the two branches of the comet's hyperbolic orbit would look like straight lines of approach and recession.

Comets which happen to pass very near a planet, however, are deflected, swirled round, and often virtually caught by that planet, receding only with an insignificant differential velocity which is unable to carry them away from the attraction of the sun: into which they often drop. Or if they do not actually drop into it, they will continue to revolve round it in an elliptic orbit, becoming a member of the solar system, and liable ultimately to be degraded into a swarm of meteors.

This is the sort of process known to occur in astronomy, and circumstances *not unlike that* may attend the encounter or apparent

collision of a furiously-flying comet-like electron with part of the massive system of an atom.

The stoppage, therefore, will occur well within the limits of atomic magnitude, 10^{-8} centimetre; and so the acceleration will be of the order $\frac{u^2}{2l} = 10^{26}$ c.g.s., and the force needed thus to stop even a single electron will be the tenth of a dyne.

No wonder that violent radiation-effects are produced. The "power" required to stop an electron, flying with one-thirtieth of the speed of light, inside a molecular thickness, can be estimated thus—

$$\text{energy} \div \text{time} = \frac{1}{2} m u^2 \cdot \frac{u}{2l} = 10^{-27}\left(10^9\right)^3 10^8 = 10^8 \text{ ergs per second;}$$

or thus—

$$F l \div t = \frac{1}{2} F u = 10^{-1} \times 10^9 = 10^8 \text{ again,}$$

which is equivalent to ten watts. (Though the time it lasts is only the 10^{-17} part of a second.)

But only a small fraction of this goes into radiation. The radiating power can be estimated thus, from Larmor's expression for it, as given in Appendix G, Part IV.—

$$\frac{\mu e^2}{v} (\dot{u})^2 = \frac{10^{-40}}{10^{10}} \times 10^{52} = 100 \text{ ergs per second.}$$

The rest therefore, it would appear, must take the form of heat.

It is worth considering what circumstances would give radiation an advantage over heat, and *vice versâ*. Because sometimes conspicuously the target gets heated, and sometimes X-rays are emitted. Let u be the speed and l the distance of stoppage, then—

$$\dot{u} = \frac{u^2}{2l},$$

then the force required to stop it is—

$$m\dot{u} = \frac{2\mu e^2}{3a}\frac{u^2}{2l}.$$

The "power" of the blow is—

$$\tfrac{1}{2} F u = \frac{\mu e^2 u^3}{6al},$$

whereas the radiation power is—

$$\frac{2\mu e^2}{3v} \cdot \left(\frac{u^2}{2l}\right)^2 = \frac{\mu e^2 u^4}{6vl^2};$$

therefore $$\frac{\text{radiating power}}{\text{total power}} = \frac{a}{l} \cdot \frac{u}{v} = \frac{2a}{vt},$$

where t is the time of stoppage, and v is the velocity of light.

So effective radiating power depends chiefly on very sudden stoppage, and on the speed being near that of light. If the velocity is a tenth that of light, and if an electron can be stopped in something like its own diameter, about 10 per cent. of the energy will go in radiation, and the rest will take other forms, presumably heat.

As the velocity diminishes, more and more of the energy takes the form of heat; which agrees with the fact that at moderate vacua the target gets red-hot.

The ratio of the radiation power to the total power is as the dimensions of an electron to the distance light would travel during the period of the stoppage. So to get *all* the energy radiated it is necessary to stop a pellet moving with a tenth the speed of light in something like a tenth of its own diameter.

Justification for Electric View of Matter.

But now what justification is there for the extraordinarily far-reaching hypothesis that the electrons constitute matter, that atoms of matter are composed of electric charges, that the fundamental inertia-property of matter is identical with self-induction?

There is the reasonable philosophical objection to postulating two methods of explaining one thing. If inertia can be explained electrically, from the phenomena of charges in motion, it seems needless to require another distinct cause for it also. But this is not all that can be said; it is quite possible that direct experimental proof will be forthcoming before long. A method suggested by Professor J. J. Thomson had reference to the proportion of radiation to thermal energy developed when corpuscles encounter a target which suddenly stops them. In so far as they consist of non-electric matter they would produce only heat by their dead collision, without any direct generation of ethereal waves. In so far as they consist of electric charges they would disperse a certain amount of radiation energy; and so the proportion of radiation to heat might afford a criterion.[1] Hitherto, however, no adequate measurements have been made in this direction.

But there is another more likely avenue to a conclusive result. We know that when an electric charge moves with a speed approaching that of light, its inertia is theoretically no longer constant, but rapidly increases and becomes infinite when the light-velocity itself is reached, at least on the orthodox and accepted theory; and rather complicated and not quite accordant expressions for this high-speed inertia have been calculated by several mathematical physicists. See Appendix K for a discussion of this difficult subject.

It is possible that in certain cases of the production of cathode rays a speed not far short of that of light may be reached, and the increased inertia observed. Such an experimental determination has been seriously and quite recently undertaken by Professor Kaufmann,[2] who employed the method indicated above (Part III.) of comparing simultaneously the

[1] See J. J. Thomson, *Phil. Mag.*, April, 1899, p. 416.
[2] See *Comptes Rendus* for October 13, 1902.

electric and the magnetic deflection of the same set of rays submitted alternately or simultaneously to an electric and a magnetic field. Thus the velocity and the e/m ratio are both known, and Kaufmann concluded that when the speeds approached perceptibly near the velocity of light the electrochemical equivalent m/e increased by just the amount required in accordance with pure electric theory—the theory which attributes the whole of inertia to electric influence. There appeared to be no quantitative room for any extra inertia, such as that of an inert particle of non-electric matter travelling with each projectile, retaining its inertia constant at all speeds, and so contributing nothing to the rise of inertia perceived when the speed approaches within hail of that of light.

It is too soon to be sure whether these results are trustworthy or not. The attempt is brilliant, and it can hardly be doubted that before long evidence will be forthcoming, on this and on other lines, which will enable us to accept or reject the hypothesis of the electric nature and unification of matter.

Meanwhile the hypothesis is in itself so probable that it is justifiable to attempt to look ahead and observe some of the consequences of the view that all atoms of matter are built up of the same fundamental units, and are composed of aggregates of a definite number of variously grouped negative and positive electrons, arranged in kinetic patterns and keeping apart by reason of the vigour of their own orbital motions.

At first it is not easy to do more than imagine the electrons to be statically grouped into regular patterns: arranged it may be in triangular or square or hexagonal order; with other allied three-dimensional possibilities familiar to students of crystallography. See, for instance, William Barlow, *Brit. Assoc. Report* 1896, p. 731; also Lord Kelvin, *Phil. Mag.*, March, 1902, and elsewhere.

ON CHEMICAL AND MOLECULAR FORCES.

The force of chemical affinity has long been known to be electrical. Ordinary electrical attraction between charged bodies may be called molar chemical action; only there is no combination in ordinary cases, because the opposing charges spark into one another, and so the attraction ceases when a certain proximity is reached. The idea that chemical forces are really electrical is as old as Sir Humphry Davy.

Real chemical attraction occurs between two atoms, each of which contains an odd number of electrons: one extra, or it may be more than one extra, electron of given sign. Such an atom has a centre of force whereby it can attach itself to other atoms and enter into pairing or chemical combination with them. It is probable that a negative charge is an excess, and a positive charge a defect; and that when pairing occurs the excess charge of one fills up the deficiency of the other, and composes a complete and neutral molecule.

Union of this kind, however, never seems quite as strong and permanent as the union of the electrons in the atom itself: the molecule easily separates at the same place again under the influence of decomposing influences, and does not seem able to split up in other ways into new substances; except in organic chemistry, where various

modes of splitting up a complex molecule can be brought about, and are practically utilised for the generation of new compounds, *e.g.*—

$$CH_3 . CH_3 = C_2H_5 . H.$$

It is probable that the same sort of thing is *possible* with simple bodies, but that the so-called "elements" constitute a peculiarly stable group, the ingredients of which so far have only partially been re-associated into isomeric or allotropic forms, and have not yet been detached from each other.

When chemical combination occurs between two oppositely charged atoms, there is no electric discharge between them: the two atoms retain each its own charge, and cling together for that reason. When they are separated, each is an ion and possesses its appropriate charge.

It is possible to charge an assemblage of neutral molecules with an excess or with a defect of one or more electrons, by processes of ordinary electrification, but the attachment of these supernumerary electrons is loose—and they can be shaken away by the agitation of ultra-violet light and in many other ways. Even splashing of liquids into spray shakes some loose.[1] And in the case of massive molecules their mutual collision or agitation under the influence of ordinary temperature is sufficient to shake away some of the loose electrons, which then fly off tangentially with whatever orbital velocity they may have had: giving rise to phenomena recently discovered under the name of *radio-activity* (see Part VII.).

Molecular Forces, Cohesion.

But there is another kind of adhesion or cohesion of molecules, not chemical but what is called molecular. This occurs between atoms not possessing ionic or extra charges, but each quite neutral, consisting of paired-off groups of electrons. At any moderate distance the force of attraction between paired electrons will be next to nothing, but at very minute distances it may be very great, ranging up to something almost indistinguishable from chemical combination, except that the cling will be a weak cling at a multitude of points instead of an intense cling at only one.

Consider the outer surface of an atom consisting of a regular group of interleaved electrons of alternately opposite sign. Its equipotential surfaces will be dimpled or corrugated or pimply sheets, which at a little distance away will be almost plain, the dimples increasing rapidly in depth and becoming like the cover of a mattrass when something less than molecular distance, something approaching the internal electron distances apart, is reached.

Two such atoms will therefore tend to settle down with their equipotential surfaces adjusted into uniformity, the pimples of the one fitting into the hollows of the other; and this is the state of things suggested by the facts of cohesion (Fig. 5).

To investigate the actual law of force would be difficult, and too

[1] Lenard on electrification near waterfalls.

many assumptions would have to be made for the geometrical arrangement of the electrons in the adjacent atoms; it could only be approximate, because we should probably, at least in the first instance, have to assume a static distribution. Nevertheless the attempt might be instructive, and might in a developed form be suitable for an Adams Prize Essay.

It is quite plain, however, that the result would be a force rapidly increasing and becoming great at small distances, and practically nil at any perceptible distance.

Molecular forces on this view are electrical, just as much electrical as are chemical forces; but they occur between chemically saturated molecules, and are due to the interaction or distant influence of paired electrons on each other across molecular distances.

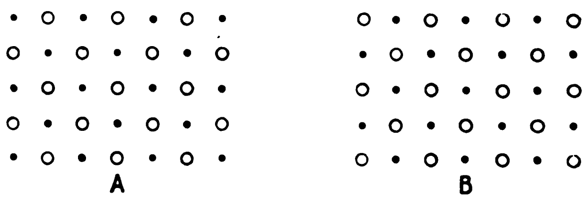


FIG. 5.—Ordinary Cohesion between two Neutral Atoms A and B: each atom consisting of interleaved electrons of opposite sign, depicted in any convenient way.

Ions cannot thus combine; because if they were oppositely charged their combination would be chemical, and if they were similarly charged they would strongly repel each other.

But if ions arrive at a metallic electrode, or are provided with other means of passing on their free charges, they cease to be ions and then they can and do combine molecularly with each other.

It is of course possible for an ion to have more than one free electron, forming a dyad or a triad radical; and the way in which a neutral group can receive and by rapid re-adjustment pass on an extra foreign electron, reminding one of the re-adjustment of the films in a lather when one compartment bursts, is doubtless instructive.

The effect of electric polarisation on such a neutral group of electrons is noteworthy. The effect of a charged body in the neighbourhood is at once to disturb the equilibrium and to disturb the grouping throughout the atom more or less: it will cause the negative electrons to protrude slightly on one side and the positive on the other (see Fig. 6).

If two molecules were beyond each other's molecular range, and if the neighbouring surfaces could by any means, by the supply of electricity from without, be oppositely electrified, the forces of cohesion would be intensified momentarily by something akin to chemical forces, and cohesion would set in over ultra-molecular distances. This *appears* to be what goes on in a "coherer." The opposite charges *cannot be maintained* electrostatically between two neighbouring

metallic surfaces, but they can be imparted with a sudden jerk or disruptive discharge or received electric impulse; and these are the things which are effective in promoting cohesion.

In the diagram herewith, Fig. 5 represents a couple of atoms with interleaved electrons of opposite sign in square order, the atoms being within range of one another and so cohering by molecular or non-chemical forces. They have adjusted themselves into a cohering position; but a shear through half the distance apart of the electrons would disintegrate them. An angle represented by half the electron-distance divided by the molecular-distance is therefore a measure of the maximum distortion of a substance.

Fig. 6 shows a couple of atoms both electrically polarised, as by a positively charged rod held above both. The constituents of C are polarised into hexagonal order — an effect such as might also be caused by lateral pressure in some cases; the constituents of D are in diagonal square order: which has the effect of violent electric polarisation. The atoms C and D are therefore clinging by forces much stronger than ordinary cohesion at that distance would have been. They represent adjacent atoms of a momentarily polarised coherer.

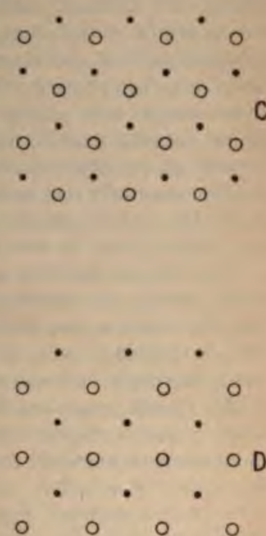


FIG. 6.—Two Polarised Atoms, polarised in different ways; illustrating also electrically intensified cohesion.

It is not to be supposed that the electrons need really ever be disturbed more than an almost imperceptible amount in order to produce this chemical cohesion effect.

APPENDIX TO PART VI.

APPENDIX K.

NOTE ON THE BEHAVIOUR OF A CHARGE MOVING NEARLY AT THE SPEED OF LIGHT.

Mr. G. F. C. Searle has kindly called my attention to a paper of his in the *Philosophical Magazine* for October, 1897, and maintains that a charge does not re-distribute itself on a moving body when its speed becomes great, but that the lines of force bend or are deflected towards the equator, without remaining normal to the surface whence they start. And I see that Mr. Heaviside in his Electrical Papers, vol. 2, p. 514, accepts this result and considers that we have no guarantee that at these high speeds lines of force are necessarily normal to conducting surfaces; an assertion in which we may trace some analogy to the fact *that in a moving* medium rays of light are not *perpendicular to their wave fronts.*

There is no question but that the lines of force bend back towards the equator, as stated by me in Part I., but I assumed that this deflexion of the lines would entail their moving up nearer to the equator of the sphere, so as to leave the poles bare of charge, in order that they might still continue radial. I admit that the lines of force need not continue radial, but it seems to me that there is still some redistribution of the charge as the speed increases: a fact, however, which is not important. Mr. Searle calculates that whereas a sphere at rest acts as if its charge were at a central point, this equivalent point opens out into a uniformly charged line, forming a medial and small portion of its diameter, when the sphere is in motion; the length of the line gradually increasing until the speed equals that of light, when it fits the sphere exactly. But this neglects a distortional change in the sphere itself, to which I will presently refer.

The fact is that the whole subject of the behaviour of a charged body moving at enormous speed is a complicated one, and directly you get within 20 per cent. of the speed of light it begins to be necessary to consider even its inertia, as well as its deflecting force, in a more thorough and elaborate manner.

Mr. Searle points out that three different estimates of inertia can be made:—one as the ratio of force to acceleration, another as the ratio of momentum to velocity, and a third as the ratio of kinetic energy to half the square of velocity. In ordinary matter as is well known, and for slow electric motions, these three estimates are one and the same; but for violent electric motions they become different; though it should be realised how small the difference is, until the speed of light is very closely approached; so that in no material case of great velocity or great acceleration that has ever been practically dealt with—as, for instance, the case of a cannon-ball stopped by armour plate—is any sort of unusual effect to be expected; even on the hypothesis that matter is entirely electrically composed. Nevertheless, now that among free corpuscles in a vacuum tube it is becoming practically possible to attain these high speeds, and even to begin to base crucial determinations upon them, it becomes necessary to consider the matter more carefully. And in a book published at Göttingen in January, 1902, Dr. Abraham discriminates what he calls "longitudinal" from what he calls "transverse" inertia; making inertia depend not only on the speed of motion, but on the direction in which the body is being accelerated.

And all these results are still further complicated by a consideration of the effect of acceleration itself, which, whenever it is violent, gives rise to some perceptible radiation, involving dissipation of energy; and this radiation loss of energy, though it will be primarily represented in the motion as a resistance or velocity term, may secondarily have an effect on inertia;—probably, however, quite a small and subordinate effect in all practical cases, and no effect at all so long as motion occurs with uniform speed in a straight line: for then there is no radiation. But then, of course, under those conditions it is not possible to test or measure the inertia of a body; it is only when the motion is either curved or changed in some way that inertia becomes prominent, and

then there is necessarily some, though usually very small, radiation too.

A convenient expression for the inertia of a body moving at any speed is hard to arrive at. A variety of expressions have been given, and some of them are contained in the first chapter to J. J. Thomson's "Recent Researches in Electricity and Magnetism," but they are not attractive, and it appears from Mr. Searle's results that it is only under very careful qualifications that they apply. For these reasons I do not propose to enter upon a discussion of them further than may be necessary to criticise or appreciate Kaufmann's recent experimental attempt at basing an important measurement on these high velocities, by observing the electric and magnetic deflexions then exhibited by cathode rays, so as to obtain if possible their modified m/e ratio at ultra high speeds.

When magnetic deflexion is being observed at ultra-high speeds we have also to remember that it is possible for the ordinary expression for the force exerted on a current by a moving field to be departed from.

The ordinary expression for deflecting force is $e\,u\,\mathrm{H}$ at low speeds, for a charge e moving at speed u across a magnetic field of intensity H; but at higher speeds an expression of much more complicated character is investigated by J. J. Thomson in the first chapter of his "Recent Researches in Electricity and Magnetism," and is obtained in the following form:—

$$e\,\mathrm{H}\,\frac{v^2-u^2}{u}\left(\frac{v}{2u}\log\frac{v+u}{v-u}-1\right)$$

This, however, at low speeds reduces not to the usual simple value, but to one-third of that value, viz. $\frac{1}{3}\,\mathrm{H}\,e\,u$; and Professor Schuster in the *Philosophical Magazine* for January, 1897, calls attention to the variety of numerical estimates of this quantity given by different varieties of the main theory.

Abraham's value of what he calls "transverse inertia" is quoted by Kaufmann in *Comptes Rendus*, vol. 135, p. 577, writing it with m_0 as the equivalent inertia for slow motion, and with β as the ratio u/v—the ratio of the speed of the motion to the velocity of light—thus

$$m=\frac{3\,m_0}{4\beta^2}\left(\frac{1+\beta^2}{2\beta}\log\frac{1+\beta}{1-\beta}-1\right)$$

and this is the formula employed by Kaufmann for experimental test and verification.

Mr. Heaviside gives a still more complex expression—Electrical Papers, vol. 2, p. 514—for kinetic energy; viz. an expression equivalent to $\frac{1}{2}\,u^2$ multiplied by the following quantity,

$$\frac{\mu e^2}{a}\cdot\frac{1-r}{4r}\left(1+\frac{2r-\frac{1}{2}}{1-r}+\frac{\left(2r-\frac{1}{2}\right)\tan^{-1}\sqrt{\left(\frac{r}{1-r}\right)}}{\sqrt{\left\{r\left(1-r\right)^3\right\}}}\right)$$

which therefore may be taken to represent the inertia for that case; r being the squared speed ratio u^2/v^2.

Larmor treats the whole matter from a general point of view in *Phil. Trans.*, 1895, p. 717, and shows that no mere acceleration term is sufficient to express completely the reaction to applied mechanical force.

Distortion due to High-speed Motion through the Ether.

In Mr. Searle's paper in the *Philosophical Magazine*, October, 1897, he points out that the simplest charged body when in motion is not a sphere, but an oblate spheroid, oblate in the direction of motion, with its axes in the ratio $\sqrt{\left(1 - \frac{u^2}{v^2}\right)}$, 1, 1; and that this produces on all points outside itself exactly the same effect as a point charge at its centre, and that therefore such a spheroid in motion at the speed u takes the place of the sphere in electrostatics. He calls this a Heaviside ellipsoid, because Mr. Heaviside first indicated its importance in the theory of moving charges.

But I wish to point out that a spheroid of this kind is exactly what a sphere in sufficiently rapid motion would automatically become, on the Fitzgerald-Lorentz theory; viz., that hypothesis which was started in order to account for the negative result in Michelson's experiment by postulating a change of dimensions in solid bodies according to their direction of motion through the ether. This hypothesis became a definite theory giving important results, when Lorentz showed that on the electric theory of matter—or even without assuming that the whole inertia of matter was electric, because the result is not a question of inertia, but of static force—not only was such a change of dimensions reasonably likely, as Fitzgerald had perceived, but that the change to be expected was precisely of the right amount to give a compensating effect and precisely zero resultant in the Michelson experiment.

The change of dimensions thus imagined and justified is gradually coming to be accepted as plausible and probably true; and it is interesting to note that a sphere in motion, by reason of being subject to this amount of distortion, still retains its property of being the simplest geometrical body, so far as the distribution of its electric field is concerned. True it is then no longer a sphere; but no measuring instrument could possibly show its distortion, because all standards of measurement would share it. It is a remarkable thing that this imperceptible and unmeasurable uniform distortion of all matter should ever have been discovered: nothing but an ethereal process could have dragged it to light. Nevertheless dragged to light it has been, by the combined testimony of electrical theory and of optical experiment.

PART VII.

Summary of Other Consequences of Electron Theory.

Radio-Activity.

If many atoms of a substance have electrons attached to them, and if these are performing orbital revolutions, it is natural to ask how then can it be that substances are not constantly emitting waves and radiating away their energy. Fortunately owing to the brilliant researches of Becquerel, Curie, and others, certain substances have been found in which the radiation intensity reaches a very perceptible magnitude; and it appears that this radiation may be of several kinds—

1st, of waves or pulses analogous to Röntgen radiation, probably;

2nd, of rays analogous to Lenard or cathode rays consisting of electrons and ions bodily shot off, certainly;

3rd, of detached portions of the substance itself not charged with electricity, but emanating like an odour, and possessing like the rest of the substance an intrinsic radiating power, and capable of attaching itself to other materials in the neighbourhood so that they too acquire temporary radiating power.[1]

The substances which possess any noteworthy amount of this radiating power are substances with very high atomic weight, and their emitting power would appear to be probably due to an internal commotion and collision between the atoms, of sufficient violence to detach, and as it were evaporate every now and then, some of the smaller particles; and also by the shock of the collisions to generate some feeble Röntgen rays.

It is easy to grant that whenever there are actual collisions of sufficient suddenness some radiation of this kind must be emitted; but we cannot help asking, why does not the quiet orbital revolution of electrons round atoms, in a substance not in a high state of thermal disturbance and not possessing specially massive atoms, why does not this also give rise to a perceptible amount of radiation and loss of energy? One answer that has been given is as follows:—

The radiators are not isolated or independent, and surface radiation is maintained by layers at greater depth in the substance. Moreover the radiators are so close together that they are in all sorts of phases within the first quarter wave length, a length which embraces a multitude of them; wherefore a multitude is a worse radiator than one, because they interfere and produce but little external or distant effect; like the two prongs of a fork, or two neighbouring organ pipes, or the front and back of a vibrating wire. See Larmor, *Ether and Matter*,

[1] See, for instance, papers by the original discoverer, M. Henri Becquerel, in *Comptes Rendus*, 1896 and 1897; see also Rutherford, *Phil Mag.* January, 1899 and 1900, with quantitative determinations concerning it. Also in *Phil. Mag.* July and November, 1902. Other references are M. and Mme. Curie, *Comptes Rendus*, November, 1899; Hon. R. J. Strutt, *Phil. Trans.* A 1901, vol. 196, p. 525; Sir W. Crookes, *Proc. Roy. Soc.*, vol. 66, p. 409 (1900), vol. 69, p. 413 (1902), also "Electrical Evaporation," 1891, *Proc. Roy. Soc.*, vol. 50, p. 88; and many other workers.

page 232. But I doubt if much answer is wanted, save one of a very different character, viz., that radiation of a low temperature order is as a matter of fact always going on from all substances; that energy is conserved and constancy of temperature persists merely because loss is equal to gain, because absorption compensates radiation, not because radiation ceases; and that to make an estimate of the amount of radiation so occurring it would be necessary to suppose the body in an enclosure at absolute zero: when undoubtedly its kinetic energy *would* rapidly leak away, and be dissipated. The whole subject of radio-activity is a large one, upon which I do not propose to enter here and now. Suffice it to realise that any difficulty of explanation in connection with it is not the fact itself, but rather why it is not more notorious.

However, so far as the most striking and interesting excessive photographic and electric radio-activity of certain rare substances is concerned, it has been already hinted that the greater part of that does not consist so much in the emission of radiation proper—whether in the form of pulses of X-rays or any other form—as in the flinging off of particles, negatively charged particles or electrons as a rule, but also sometimes, according to Mr. R. J. Strutt, of positive ions also. The faint photographic influence of ordinary substances observed by Dr. W. H. Russell seemed to suggest that incipient power of this kind is not limited to bodies with heavy atoms like Uranium, Radium, Polonium, etc., as described by Becquerel and the Curies, though these substances show it to an extraordinary degree: Dr. Russell, however, appears to have traced his at first interesting effects to the merely chemical action of hydrogen peroxide.

The whole subject, together with the allied one of the loss of charge from hot bodies,[1] first discovered by Dr. Guthrie long ago (see *Phil. Mag.* [4], xlvi. p. 273), is one that demands special attention and treatment, for which there is no opportunity now.

Solar Corona, Comets' Tails, Magnetic Storms, and Auroræ.

Another subject on which it is tempting to enlarge is the explanation of various astronomical and meteorological phenomena by the electron theory.

The theory of Auroræ has recently been elaborated by Arrhenius; but the whole doctrine of emanations from the sun, and of repulsion of small particles both by his light and by his probable electrification, is a matter that has been familiar to me for several years, through conversation with Fitzgerald and others. See, for instance, Larmor, *Phil. Trans.* 1894, vol. 185, p. 813; Lodge on Sunspots, Magnetic Storms, Comets' Tails, Atmospheric Electricity, and Auroræ, in the *Electrician* for December 7, 1900, vol. 46, p. 250; Fitzgerald, *Electrician*, December 14, 1900, with reference to a communication on the subject in 1893 (see the *Electrician* for August 11, 1893). See also his collected "Scientific Writings," at date 1882.

The earth is in fact a target exposed to cathode rays, or rather to

[1] See, for instance, Strutt on leakage from hot bodies, *Phil. Mag.* July, 1902; *J. J.* Thomson, ditto, *Phil. Mag.* August, 1902.

electrons emitted by a hot body, viz. the sun. The gradual accumulation of negative electricity by the earth is a natural consequence of this electron bombardment, and the fact that the torrent of particles constitutes an electric current of fair strength gives an easy explanation of one class of magnetic storms; which have been long known, by the method of concomitant variations, to be connected with sunspots and auroræ. The electric nuclei would also serve as centres for condensation of atmospheric vapour at high altitudes and so be liable to affect rainfall.

Nevertheless it is true that these theories have been well elaborated of late by Arrhenius; and his explanation of the aurora by means of the catching and guiding of rapidly moving electrons by the earth's magnetic lines of force, so as to deflect them from the tropics and conduct them in long spirals, along the lines, to the poles, there to reproduce the phenomena of the vacuum-tube in the rarified upper regions of the atmosphere, is particularly definite and pleasing. Some of the other astronomical suggestions he has made are likewise of considerable interest.

Validity of Old Views.

Now that the doctrine of electricity (at least of negative electricity) as located in small charges or charged bodies is definitely accepted, and now that a current can be treated as the locomotion of actual electricity, it may seem as if some doubt were thrown upon the doctrine, which a little time ago was spoken of as a "modern view," that the energy of an electric current resides in the space round a conductor. There is no inconsistency, however. The whole of the fields of an electron are outside itself; it is in its fields that its energy resides, and it is in the space round it that energy is conveyed when it moves; for the ether in that space is subject to the co-existence of an electric and a magnetic field. So, also, its inertia resides in space round it, for it is accounted for by the E.M.F. set up when its magnetic field changes, that is when its motion is accelerated.

In dealing with the inertia of matter it is commonly supposed that the inertia resides in the matter itself: whereas electrical inertia is known to reside in the space round the nucleus. Yet we have been emphasising and opposing the view that material inertia and electrical inertia are essentially one and the same.

Is there no inconsistency here?

The appearance of inconsistency vanishes when we come to calculate and realise how extremely local and concentrated the intense part of the field of an electron is. There is a sense in which it can be said that a moving body, for instance a vortex ring, disturbs the whole atmosphere; but any perceptible disturbance resides very near the ring. So it is with an electron. The magnetic field falls off inversely as the square of the distance from the moving nucleus, and hence at a distance far less than a millimetre, less even than the size of an atom, it is quite inappreciable. The whole magnetic field on which its inertia depends lies practically very close to the electron itself: it is just its extremely small size that *enables this concentration* to be possible, and even in a

closely packed mercury atom there is practically no encroachment of the field of one electron on its neighbour's. They are all independent, each with its own inertia, almost isolated from the others: for if it were not so, the mass of a body in close chemical combination would not continue constant, but would diminish. Whether it does diminish in the least degree is a question perhaps worthy of attack.[1]

The momentum of a moving charge at ordinary speeds is simply inversely as the radius of the sphere which holds it, as stated in Part I., but the localisation of this momentum, which is the point we are now considering, is given generally in Thomson's *Recent Researches in Electricity and Magnetism*, p. 20. and may be realised approximately as follows:—

The momentum depends on the co-existence and product of the electric and magnetic fields. Each field varies inversely as the square of the distance from the moving charge; and their vector product is, as regards direction, perpendicular to the radius vector at any point, and proportional at ordinary speeds to the sine of the angle between the radius vector and the direction of motion, while in magnitude it falls off as the inverse fourth power of the distance. All this can be realised by common sense with very little trouble.

So, then, take a moving electron, and consider the distribution of its momentum in the space round it. Between its surface and a space of a hundred times its diameter, 99 per cent. of its momentum is contained; because we shall have to integrate the factor—

$$\int_a^r \frac{4\pi r^2 dr}{r^4}$$

So, within the boundary of an atom, which is a hundred-thousand times an electron's diameter, there is practically none of its momentum not included.

And even in one of the comparatively closely packed atoms, *e.g.* in a platinum or mercury atom, the overlapping of momentum for each constituent is extremely small, since their average space apart is some thousand times the size of each constituent electron.

Consequently the assertions that an electric current is a transfer of electrons, and that the energy of a current travels in the space surrounding the moving electricity, are statements not inconsistent with each other. Nor are the statements inconsistent that the mass of a body resides in its atoms, and that inertia or momentum is a property due to the self-inductive influence of the electromagnetic field surrounding a moving electric nucleus. So also with the way in which a current is propelled. The pace of progression of electrons through a solid may be considerable, see next section, but it is very far below the pace at which a telegraphic signal travels along a wire. They must be propelled by a lateral action, transmitted through the ether with the speed of light appropriate to the surrounding insulator, by some arrangement which "Modern Views" symbolised in the form of cog-wheels: they cannot be impelled by end thrust. The electric current

[1] *Cf.* Rayleigh, British Association, Belfast 1902.

is a more material entity, or has a more nearly material aspect, than was thought probable a little while since; but all that was taught about its mode of propulsion and the diffusion of the propelling force from outside to inside through successive layers, as it were, of the wire, all that was taught about the paths by which the energy travels and arrives at point after point of the wire, there to be dissipated as heat, remains true.

Number of Ions in Conductors.

The immense number of electrons that are necessary to make up the mass of a piece of platinum, or of a lump of matter like the earth, can readily be estimated; so, also, it is easy to imagine that an enormous number must be travelling in order to give customary strengths of current such as can readily pass through a liquid.

Through a gas a limit is soon found to the available number, and accordingly the conductivity of an ionised gas falls off if we call upon it to carry more than a certain current, called the saturation current. See investigations by Townsend and others. But I am not aware of any experimental indication of such a limit in solids or liquids at present. In solids the pace of travel is unknown, though it has been ingeniously surmised, and is thought to be very great; considerations of centrifugal force would make the speed of each electron during an atomic encounter equal to $e/\sqrt{(\mathrm{K}\,m\,r)}$ or about 10^8 centimetres per second; views based on Maxwell's theorem about equal distribution of energy among the particles of mixed gases suggest 10^7 for the average speed of electrons at ordinary temperatures in a solid where they were free, that is a hundred kilometres or sixty miles per second; though, since each particle is subject to constant changes of direction, this is by no means the pace of straightforward *progression*. But in liquids they are attached to atoms, and the pace of progression is known both theoretically and experimentally with considerable accuracy, and is comparable to an inch an hour for customary gradients of potential.

The total current is neu; and to give a unit c.g.s. current at so low a speed we can reckon how many ions there must be.

For $e = 10^{-20}$ electromagnetic units;

so if we take $u = 10^{-3}$ centimetre per second,

then the number of ions engaged in conveying the c.g.s. unit of 10 amperes is $n = 10^{23}$. But, after all, this is nothing very great. It is only about the number of atoms in a cubic centimetre of liquid, and by applying a greater gradient of potential the ions can be made to move faster. By gradually narrowing down the section of a liquid conductor under a given gradient of potential, it might seem possible to get evidence of an approach to a saturation-current-density in liquids. The observed accuracy of Ohm's law[1] under such conditions, however, is against this experimental possibility.

Conclusion.

The subject is very far from exhausted, but I must not attempt to cover more ground. The most exciting part of the whole is the

[1] *Fitzgerald and Trouton, Brit.* Assoc. Reports, 1886, 1887, 1888.

explanation of matter in terms of electricity, the view that electricity is, after all, the fundamental substance, and that what we have been accustomed to regard as an indivisible atom of matter is built up out of it; that all atoms—atoms of all sorts of substances—are built up of the same thing. In fact the theoretical and proximate achievement of what philosophers have always sought after, viz., a *unification of matter.* And another surprising and suggestive result is that the spaces inside an atom are so enormous compared with the size of the electrical nuclei themselves which compose it; so that an atom is a complicated kind of astronomical system, like Saturn's ring, or perhaps more like a nebula, with no sun, but with a large number of equal bodies possessing inertia and subject to mutual electric attractive and repulsive forces of great magnitude, to replace gravitation. The radiation of a nebula may be due to shocks and collisions somewhat like the X-radiation from some atoms.

The disproportion between the size of an atom and the size of an electron is vastly greater than that between the sun and the earth. If an electron is depicted as a speck one-hundredth of an inch in diameter, like one of the full-stops on this page for instance, the space available for the few hundred or thousand of such constituent dots to disport themselves inside an atom is comparable to a hundred-feet cube; in other words, the atom on the same scale would be represented by a church 160 feet long, 80 feet broad, and 40 feet high, in which therefore the dots would be almost lost. And yet on the electric theory of matter they are all of the atom that there is; they "occupy" its volume in the sense of keeping other things out, as soldiers occupy a country; they are energetic and forceful though not bulky, and in their mutual relations they constitute what we call the atom of matter; they give it its inertia, they enable it to cling on to others which come within short range, and by excess or defect of one or more constituents they exhibit chemical properties and attach themselves with vigour to others in like or rather opposite case.

That such an atom, composed only of sparse dots, can move through the ether without resistance is not surprising. They have links of attachment with each other, but so long as the speed is steady they have no links of attachment with the ether; if they disturb it at all in steady motion it is probably only by the simplest irrotational class of disturbance which permits of no detection by any optical means.[1] Nor do they tend to drag it about. All known lines of mechanical force reach from atom to atom, they never terminate in ether; except indeed at an advancing wave front. At a wave front is to be found the reaction of a mechanical pressure of radiation whose other component rests on the source. This is an interesting but essentially non-statical case, and it leads away from our subject.

As to the nature of an electron regarded as an ethereal phenomenon, it is too early days to express any opinion. At present it is not clear why positive electrons should cling so tenaciously to a group, while an outstanding negative electron should readily escape and travel free.

[1] See *Phil. Trans.* 1893, vol. 184, pp. 750–754; also vol. 189, p. 166.

Nor is the nature of gravitation yet understood. When the electron theory is complete to the second order, or some higher *even* order, of small quantities, it is hoped that the gravitative property also will fall into line and form part of the theory; at present it is an empirical fact which we observe without understanding; as has been our predicament not only since the days of Newton but for centuries before.

Attention has hitherto been chiefly concentrated on the freely-moving active negative ingredient,—the more sluggish positive charges are at first of less interest,—but the behaviour of electrons cannot be fully and properly understood without a knowledge of the nature and properties of the positive constituent too.

The positive electron has not, so far as I know, been as yet observed free. Some think it cannot exist in a free state, that it is in fact the rest of the atom of matter from which a negative unit charge has been removed; or, to put it crudely—that "electricity" repels "electricity," and "matter" repels "matter," but that Electricity and Matter in combination form a neutral substance which is the atom of matter as we know it. Such a statement is an extraordinary and striking return to the views expressed by that great genius, Benjamin Franklin. On any hypothesis those views of his are of exceeding interest, and show once more the kind of prophetic insight which we have had occasion to notice in discoverers before (Appendix H above). Undoubtedly we are at the present time nearer to the view of Benjamin Franklin than men have been at any intervening period between his time and ours.

The view that an atom is composed of an equal number of interleaved or inter-revolving positive and negative electrons—to which it will have been observed I myself tentatively and provisionally incline—that view is not Franklin's; nor is it as yet anything but a guess. To make it more, work must be done upon the nature and properties of the positive charge; and the positive electron, if it exists, must be dragged experimentally to light.

Especially must the inner ethereal meaning both of positive and negative charges be explained: whether on the notion of a right-and left-handed self-locked intrinsic wrench-strain in a Kelvin gyrostatically-stable ether, at present being elaborated by Larmor,[1] or on some hitherto unimagined plan. And this will entail a quantity of exploring mathematical work of the highest order.

The PRESIDENT: I have heard the suggestion made that there might be one or two people in this Institution who do not think electrons are things worth troubling about, but we must remember that the subject which has been dealt with to-night is the very basis of modern science. The question of electrons is to us by far the most important question of the day. Electrons are hypothetical bodies which help us to think straight. We owe a great debt of gratitude to Sir Oliver Lodge for coming here to-night. He has really only done his duty, because it is the duty of any one who is—to use Sir Oliver's The President.

[1] See *Ether and Matter*, p. 326; or *Phil. Trans.* 1894, pp. 810, 811, and 1897, pp. 209–212.

own expression—a pioneer of Science, to come and help other people on by explaining to us from time to time how far he has got. But Sir Oliver Lodge, in addition to doing his duty, is able to do his duty exceedingly well. There are very few people who not only understand a very difficult subject but who also can translate it into English. Some time ago Jevons remarked that the elementary edition of Thomson and Tait's "Natural Philosophy" was quite as mathematical as the fat volume we know so well. What he probably meant was that it took just as much genius (if not more) to write an apparently elementary treatise in plain English as it did to write a book using mathematical expressions. Sir Oliver Lodge is admirable in his exposition of an exceedingly difficult subject, and not only this Institution but the whole of the scientific world who are interested in these matters owe a debt of gratitude to him for giving us a paper which will enable us to some extent to get our knowledge up-to-date in the most important branch of science that there is at the present time. We all remember Sir Oliver's *Modern Views On Electricity*. That book is going to be something like the *Encyclopædia Britannica:* it is going to have supplements from time to time bringing it up-to-date. I will now put to the meeting that we pass a cordial vote of thanks to Sir Oliver Lodge.

The vote was carried by acclamation.

Sir OLIVER LODGE: I am very much obliged to you, gentlemen. I would also like to express my thanks to Sir William Crookes and also to Mr. Gardner for having taken the trouble to bring the apparatus for the purpose of illustrating the paper.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Members.

Kay Oscar Arthur Gulstad.	Wilfred James Lineham.
Edward George Jones.	David Reid.

Arthur Wilkinson Whieldon.

Associate Members.

Sydney Ernest Britton.	Charles Wheusa Nicholl.
Walter Charles Brown.	Henry William John Peterson.
William Woodyer Buckton.	Percy Edward Rycroft.
William Edward Beck Dove.	William Hugh Smith.
Alfred Lindsay Forster.	Gilbert Richard Spurr.
George Edward Heyl-Dia.	William Wharam.
William Mannox.	Harold Langton Tyson Wolff.

Associates.

Herbert Bailey.
Alfred William Bennis.
Eric Francis Boult.
Charles Borthwick Chartres.
Harry De Pinna.
George Dixon.
Henry Benjamin Dorrell.
John Francis Edmonds.
Alfred Eve.
John Gilligan.
Albert Gray.
Samuel Barnes Griffith.
Louis Thomas Healy.
Francis Christian Heritage.
Archibald Johnston.
Louis J. Lawless.
George Catterall Leach.
Arnold B. Longden.
William McDonald.
James McLachlan.
Warwick Makinson.
William Henry Merrett.
Ernest Henry Mottram.
Ernest Holt Owtram.
Percy Claude Parker.
Richard Rigg.
Ernest Castle Roche.
John William Percy Scott.
William Bellhouse Scott.
Charles Henry Shanan.
Edward Vernon Flamank Shaw.
Hugh Christopher Silver.
Albert Smith.
Thomas Smith.
James Daniel Stevens.
Walter William Wakley.
Frank Walker.
Harold West.
Cecil Harington Williams.
James Cooper Wilson.
Robert Ernest Workman.

Students.

Nai Barr.
William Blathwayt.
James Bollands, jun.
Richard England Brooke.
Edward Fisher.
Victor William Gill.
Jeremiah Hague.
Ralph Hardy.
Ralph Pacey Hulton.
Philip Vassar Hunter.
Philip Henry Keeling.
Richard Line.
Thomas Mason.
William Harry Maystone.
William George Perry.
Ernest William Porter.
Ernest Byers Thomas.
Egerton John Ward.
Reginald Choldmeley Campbell Yates.

THE Three Hundred and Eighty-third Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, December 11th, 1902—Mr. JAMES SWINBURNE, President, in the chair.

The minutes of the Ordinary General Meeting held on December 4th, 1902, were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that these names should be suspended in the Library.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Associate Members.

Augustus Archer.
Henry Martin Bayly
Robert John Halliburton Beaty.
Harry Jeffery Bellow.
Harold Bentham.
Herbert Carpmael.
Frederick Samuel Carter.
Herbert John Coates.
W. J. Coles.
Arthur Douglas Constable.
John Frederic Coote.
Frank Whinfield Crawter.
Jas. Mountjoy Elliott.
James S. Enright.
Louis Henry Euler.
Oswald Lofthouse Falconar.
Chas. Walter Fourniss.
Archibald John French.
Arthur Thomas Gordon-Smith.
Wm. Jas. Grey.
James Hall.
Sydney Enoch Hall.
Patrick Hamilton.
Lionel Edward Harvey.
Alfred William Hill.
Frederick Hutchins.
Frederic Osmond Hunt.
Julius Pierpoint Lawrence.
Arthur Hector Lidderdale.
Henry A. Lewis.
Richard Percy Lovell.
G. A. Maquay.
William C. Martin.
Alick James Newport-Kennett.
Edwyn S. Pope.
Douglas Potter.
William Pearson Richmond.
George Weston.

Messrs. F. B. O'Hanlan and A. K. Taylor were appointed scrutineers of the ballot for the election of new members.

A Donation to the *Library* was announced as having been received since the last meeting from Mr. R. T. Atkinson, to whom the thanks of the meeting were duly accorded.

The following paper was read :—

THE PHOTOMETRY OF ELECTRIC LAMPS.

By Dr. J. A. FLEMING, M.A., F.R.S., Member, Professor of Electrical Engineering in University College, London.

Although a large number of the Members and Associates of this Institution are connected with electric lighting, and therefore unquestionably interested in the efficiency of electric lamps as light-producing agents, it is somewhat remarkable that in the last twenty years we have had only one discussion on the subject of Photometry.[1] We have had many papers on the use and physics of incandescence and arc-lamps, and the means for measuring the energy supplied to them, but not one exclusively devoted to the processes for determining their photogenic value. We frequently see glow-lamp efficiencies expressed in figures running to two decimal places, yet it needs but little acquaintance with the subject of light-measurement to compel an admission that the probable accuracy of the determination of the illuminating power is not often sufficient to justify it.

The following incident in this connection, which came under the personal notice of the writer a few months ago, is significant. A certain firm ordered from well-known manufacturers of glow-lamps some special lamps which were to be carefully marked for volts and candle-power. These lamps when delivered were submitted to another testing laboratory for verification, and the difference between the candle-powers affirmed by the manufacturers and those given by the testing laboratory amounted in some cases to 25 per cent. In another instance, some lamps were brought over from the United States, which were stated to be 16 c.p. at a certain voltage. They were sent to a lamp factory in London to be tested for candle-power at the marked voltages, and the candle-powers were returned in all cases at numbers between 18 and 19. In other words, there was a difference of about 20 per cent. between measurements made in New York and those made in London. These differences may have been to some slight extent due to the electrical measurements, but there is no question that the principal part of the error was in the photometric determinations. These measurements were not made by careless observers, but by competent persons, and the facts show that no excuse is needed for again bringing the subject of the photometry of electric lamps before this Institution.

If such variations exist in the case of incandescent lamp tests, they indicate that many arc-lamp candle-power and efficiency measurements, which involve all the difficulties of heterochromatic photometry, may be even more uncertain in value.

The exact marking of glow-lamps with their actual candle-power is important, because otherwise comparisons between various makes of

[1] "On a New *Form of Portable* Photometer," by Sir David Salomons (and *subsequent discussion), Journal Inst. Elec. Eng.*, vol. 22, p. 197 (1893).

lamps are misleading. In the case of arc-lamps the value of improvements cannot be properly estimated if one of the factors in the efficiency is uncertain within wide limits.

The subject is therefore ripe for discussion, and the more so because the Metropolitan Gas Referees, a body of experts appointed to control the testing of London gas, not long ago made important alterations in the official methods of gas-light photometry. These changes have only been made after prolonged inquiries, hence it is most desirable that electrical engineers should be acquainted with these methods, and that gas engineers and electrical engineers should be in agreement at least on the one subject of the unit or standard of light. Moreover, International agreement as to processes, as well as standards for electric-light photometry is required, since photometry when the lights are of very different spectral composition is complicated by peculiar difficulties, and hence involves the means of measurement as well as the standards of comparison.

The subject naturally divides itself into the consideration of—

1. Standards.
2. Processes of measurements.
3. Special considerations affecting heterochromatic photometry.
4. International agreements on the subject of standards of light and processes of testing.

I. Standards.

Whilst the sperm candle, six to the pound, burning 120 grains of spermaceti per hour, still retains its position as the legal standard in the United Kingdom, owing to its mention in the Metropolis Gas Act of 1860, and the Gas Works Clauses Amendment Act of 1871, it has been practically now dethroned from the position it has long unworthily occupied, by the action of the Metropolitan Gas Referees in adopting the 10-candle power Vernon Harcourt Pentane lamp as the official light standard for the testing of gas. We need not, therefore, spend a moment in abusing the Parliamentary candle. It has been extensively investigated and universally condemned.[1] Although the Gas Referees have no jurisdiction outside London, yet some large towns, such as Birmingham, Hastings, etc., follow their lead, and probably in a short time the use of the sperm candle which was once obligatory in gas-testing will have entirely ceased.

In spite of the elaborate specification for its preparation and use, issued by the Gas Referees in England, general experience shows that candle standards of any kind are inferior to other flame standards using a liquid fuel.

[1] For an exhaustive criticism of the "candle" as a standard of light, the reader may be referred to "A Report on Standards of Light presented to the American Institute of Electrical Engineers," by Prof. E. L. Nichols, and Messrs. C. H. Sharp and C. P. Matthews (see *Trans. Am. Inst. Elec. Eng.*, *vol. 8*). *See also* "A Method for the Use of Standard Candles in Photometry," *by C. H. Sharp* (*Physical Review*, vol. 3, p. 458).

The same remarks apply to the German paraffin candle, the so-called "Vereinskerze" or Association candle, once the official standard in that country; defined at the suggestion of, and its mode of use carefully specified by, the German Association of Gas and Water Engineers; but now displaced by the Hefner lamp, which has become the legal standard of light in Germany.

The standards of light or illuminating power now in use are divided into—

1. Flame standards.
2. Incandescence standards.

And we may furthermore divide them into Primary or Reference standards and Secondary or Working standards.

The flame standards which have been exhaustively investigated up to the present are :—

(*a*) The Colza oil or Carcel standard, which remains the official standard for gas-testing in France, and still preserves the form given to it by Dumas and Regnault.

(*b*) The various Pentane lamps of Mr. A. G. Vernon Harcourt, F.R.S., well known and much used in Great Britain, one of which is now the official standard for London gas-testing.

(*c*) The Amyl Acetate lamp of Herr von Hefner-Alteneck, introduced in 1884, and extensively employed in Germany, where it is the legal standard.

Other flame standards which have been suggested and more or less used are :—

(*d*) The Argand coal-gas flame with Methven slit, the coal-gas being sometimes enriched with pentane.

(*e*) The Benzene and Ether flame recommended by Dutch Photometric Commission in 1893.

(*f*) The Acetylene flame standard of Charpentier.

(*g*) The Acetylene and Hydrogen flame, two parts acetylene and one part hydrogen, burnt in pure oxygen, recommended by the American Institute of Electrical Engineers.

(*h*) The Ethylene flame, consisting of pure ethylene burning in pure oxygen, suggested by M. A. Blondel.

(*i*) The Albo-carbon lamp, burning naphthalene, proposed by M. Broca.

It is generally agreed that a flame standard must comply with three conditions :—

1. The combustible must be of constant and definite chemical composition easily obtained pure, and tested for purity without difficulty.
2. It must be burnt under simple and easily controlled conditions.
3. Unavoidable changes in atmospheric pressure and composition must not affect the character of the flame sensibly.

Moreover, it should be capable of being set up anywhere and be self-contained. This last condition rules out any coal-gas standard, even if experience has not shown that the Methven screen by no means renders the light emitted by a coal-gas flame independent of the composition of the gas.

Also in spite of the fact that the Colza oil lamp has maintained its position in France as the official standard of light for the greater part of the century, the uncertain composition of this combustible has prevented its adoption in other countries. The three flame standards which at present hold the field are :—

1. The 1-candle Pentane Reference Standard, introduced by Mr. A. G. Vernon Harcourt in 1877.
2. The more recent 10-candle Pentane lamp by the same inventor, now adopted as the official working standard by the Gas Referees, brought out in 1898.[1]
3. The Amyl Acetate lamp, introduced by Herr von Hefner-Alteneck in 1884.

Mr. Harcourt's work on Photometry, which has extended over nearly thirty years, is too well known to need eulogium, and is based upon the employment of pentane as a standard fuel. This very volatile and inflammable liquid, having the chemical composition C_5H_{12}, is the distillate yielded by light American petroleum after three distillations respectively at 55° C., 50° C., and 45° C., and subsequent treatment with strong sulphuric acid and caustic soda. The vapour of pentane is 2·5 times heavier than atmospheric air, and is as inflammable as ether. The specification for its preparation and testing is given in the Gas Referee's Notification for 1901, as follows :—

"*Preparation.*—Light American petroleum, such as known as Gasoline and used for making air-gas, is to be further rectified by three distillations, at 55° C., 50° C., and 45° C. in succession. The distillate at 45° C. is to be shaken up from time to time during two periods of not less than three hours each with one-tenth its bulk of—

(1) Strong sulphuric acid.
(2) Solution of caustic soda.

After this treatment it is to be again distilled, and that portion is to be collected for use which comes over between the temperatures of 25° C. and 40° C. It will consist chiefly of pentane, together with small quantities of lower and higher homologues, whose presence does not affect the light of the lamp.

"*Testing.*—The density of the liquid pentane at 15° C. should not be less than 0·6235, nor more than 0·626 as compared with that of water of maximum density. The density of the pentane when gaseous, as compared with that of hydrogen at the same temperature and under the same pressure, may be taken. This is done most readily and exactly by Guy Lussac's method, under a pressure of about half an atmosphere and at temperatures between 25° C. and 35° C. The density of gaseous pentane should lie between 36 and 38.

"Any admixture with pentane of hydrocarbons belonging to other groups

[1] See *Proc. British Assoc.*, Bristol, 1898, "On a 10-candle Lamp to be used as a Standard of Light," by A. G. Vernon Harcourt, F.R.S.

and having a higher photogenic value, such as benzene or amylene, must be avoided. Their presence may be detected by the following test: Bring into a stoppered 4-oz. bottle of white glass 10 c.c. of nitric acid, specific gravity 1·32 (made by diluting pure nitric acid with half its bulk of water), add 1 c.c. of a dilute solution of potassium permanganate containing 0·1 gram of permanganate in 200 c.c. Pour into the bottle 50 c.c. of the sample of pentane, and shake strongly during five successive periods of 20 seconds. If no hydrocarbons other than paraffins are present, the pink colour, though somewhat paler, will still be distinct; if there is an admixture of as much as ½ per cent. of amylene or benzene, the colour will have disappeared."

It is important to notice these precautions as to testing cannot be dispensed with. Merely to write to a wholesale chemist for pentane, or something called pentane, and then use it in a Harcourt lamp will not result in the reproduction of the standard of light. The pentane used in gas-testing is prepared in bulk by the Gas Companies, and is then tested by the Referees and supplied in sealed cans to the Gas-Testing Stations, which are under the control of Dr. F. Clowes, the Chemical Adviser of the London County Council, and Prof. Vivian B. Lewes, the Gas Examiner for the City Corporation.[1]

Mr. Vernon Harcourt has devised at various times five forms of lamp for burning pentane, three of them being 1-candle-power standards, and two of them 10-candle-power standards. The most important at the present time is the 1-candle-power standard which was introduced by him to the British Association at Plymouth in 1877.[2] The burner of this lamp consists of a brass tube 4 inches long and 1 inch in diameter, having a brass plug half an inch thick at the top, with a hole bored in it a quarter of an inch in diameter. Round the burner is placed a glass chimney 6 inches high and 2 inches in diameter, the top of which is level with the top of the burner. Air enters through holes in the gallery on which the chimney stands, and rises up round the flame. A piece of platinum wire 0·6 mm. in diameter is supported by a bracket 63·5 mm. above the top of the burner. The combustible used with this burner is a mixture of pentane vapour and air in the proportion of 3 cubic inches of pentane to 1 cubic foot of air. This mixture is made in a gas-holder in the proportion of 9 cubic inches of pentane and 3 cubic feet of air, and after standing should have a volume at a barometric pressure of 30 inches, and a temperature of 62° F. of 4 cubic feet, or more exactly between 4·02 and 4·1 cubic feet. This mixture is burned in the above jet at the rate of half a cubic foot per hour, or at a rate not exceeding the limits of 0·48 and 0·52 cubic foot per hour. The air-gas passes through a small meter and governor on the way to the jet. The height of the flame is regulated by a delicate stopcock to be 2·5 inches high, or just to touch the platinum wire.

[1] Mr. Vernon Harcourt has informed the author that this Standard Pentane can be procured from Mr. S. E. Miller, of 115, Cowley Road, Oxford, who has had experience under Mr. Harcourt's direction in making it. Messrs. Wright & Co., of Precision Works, Page Street, Westminster, who supply the latest form of 10-candle Pentane Lamp, have also undertaken to put on sale standardized pentane complying with the above specification.

[2] See *Proc. Brit. Assoc., Plymouth, 1877*, p. 51. See also Proc. Brit. Assoc., *Southport, 1883, p. 426; and Proc. Brit. Assoc.,* Bristol, 1898, p. 845.

This adjustment needs care, and in doing it the observer's eye should be screened from the general mass of the flame and see only the tip. When these operations are performed, we have a yellow-white flame produced which yields a light equal to the mean British Standard candle, but is much more constant. It need hardly be said that this Pentane lamp has to be used in a suitable position with good ventilation, but free from draughts, and there are certain corrections to be applied for variations in the atmospheric pressure and moisture and carbonic dioxide present in the air.

The effects of variations in the hygrometric state of the air and of barometric pressure on the Pentane flame have been investigated by Liebenthal and by Mr. Harcourt.[1] The latter states with reference to the 1-candle Pentane standard that the height of the cone of flame varies inversely as the barometric pressure, and he gives the following rule for the correction of standard heights of flame. The standard height of flame for which the emitted light is equal to one candle is 63·5 mm. at 30 inches barometric pressure, and for every tenth of an inch above or below 30 inches the flame must be set an equal number of fifths of a millimetre below or above 63·5 mm. Hence when the barometer stands at 30·5 inches, the height of flame to give one candle is 62·5 mm.

Liebenthal[2] examined the effect of water vapour on the Harcourt 1-candle lamp with wick, and found that its luminous intensity in term of the Hefner unit (see below) was expressed by the formula :—

$$L = 1{\cdot}232\,(1 - 0{\cdot}0055\,w),$$

where w is the number of litres of water vapour in each cubic metre dry air. The formula holds good between 4 and 18 litres.

Also he investigated the effect of atmospheric pressure, and stat that the change in the illuminating power of the Pentane lamp expressed by the rule :—

$$\Delta L = 0{\cdot}00049\,(H - 760),$$

where ΔL is the variation of light corresponding to a barometric hei of H millimetres. Thus an increase of 40 mm. in pressure results i variation of the light of 2 per cent.

These experiments were made with a form of portable 1-can Pentane lamp which was brought out some time ago, and someti called the Woodhouse and Rawson pattern, from the names of a fi who sold it. The writer is not aware whether particular experime have yet been made to determine the effect of variations of atmosph pressure, carbon dioxide, and moisture upon the luminous intensit the official 10-candle chimneyless Argand Pentane Lamp descri above. It appears desirable, however, that this information should obtained, in view of the adoption of the lamp as a standard by the Referees.

It will not be necessary here to describe in great detail all tl

[1] See *Proc. Brit. Assoc.*, Aberdeen, 1885.

[2] *Electrotechnische Zeitschrift*, vol. 3, p. 445, and vol. 5, p. 20.

operations of reproducing a standard of light with this Pentane lamp. These can be obtained from the numerous reports and descriptions of it which have already been given. After careful investigation, its use was recommended by a Committee of the Board of Trade in 1881, and by the Standards of Light Committee of the British Association in 1888. This last committee reported that the Pentane standard fulfilled all the conditions required in a standard of light. They found that the light was not altered by slight variations in the specific gravity of the pentane varying between 0·628 and 0·632. Out of 117 tests only one showed a variation of 1 per cent., and there were no larger variations. It has been demonstrated, therefore, that this standard affords a means of reproducing with an accuracy of 1 per cent. a light which represents fairly the ideal mean British standard candle. The necessity for employing the gas-holder, meter, governor, and other checking appliances renders this lamp more suitable for a primary reference standard than a working standard. These last objections, however, have been removed in the latest form of Harcourt Pentane Lamp, which is the one mentioned as now adopted by the Gas Referees. A full description of this lamp is given in the notification of the Gas Referees for 1901, Appendix A; and also in a paper by Dr. F. Clowes, Superintending Gas Examiner of the London County Council, in the *Journal of the Society of Chemical Industry*, March 15, 1902, No. 5, vol. 21.

This lamp, which is exhibited on the table before you, has a reservoir called *a saturator*, which contains pentane placed at the top of a hollow pillar. The reservoir has two openings closed by stopcocks, one to admit air and the other as an exit for pentane vapour. The pentane vapour descends through an india-rubber tube by its own weight, being syphoned off from the space above the liquid pentane in the reservoir. It is led down into an Argand burner at the base of the pillar. Over this is a double metallic chimney. The air supplied to the centre of the burner is drawn up between two concentric chimney tubes and led down the pillar to the burner, as shown in the diagram. Hence the arrangement forms a sort of regenerative burner. The chimney comes down to within a distance of 47 mm. above the steatite ring burner, the proper gap being determined by a boxwood gauge. The chimney cuts off the top of the flame, and there is a mica window in the chimney through which to observe the height of the tip of the flame. The flame is moreover surrounded by a conical metallic shield with an opening in it. This lamp is managed with great ease. All that is necessary is to put into the reservoir a pint of pentane, and then to open both stopcocks and after a few moments to light the jet of vapour at the burner, and regulate flow of air and vapour by the stopcocks until the tip of the flame is seen at the middle of the mica window. When so adjusted, the lamp gives a light ten times that of the 1-candle Pentane Standard, and is taken as the official standard of 10-candle light by the Gas Referees. It is necessary to adjust the height of the flame somewhat exactly, and to wait for the lamp to settle down to an uniform temperature before beginning observations.

The following is the official description of the lamp given by the Gas Referees in their notification for November, 1901 :—

"Mr. Harcourt's Ten-Candle Pentane Lamp is one in which air is saturated with pentane vapour, the air-gas so formed descending by its gravity to a steatite ring-burner. The flame is drawn into a definite form, and the top of it is hidden from view by a long brass chimney above the steatite burner. The

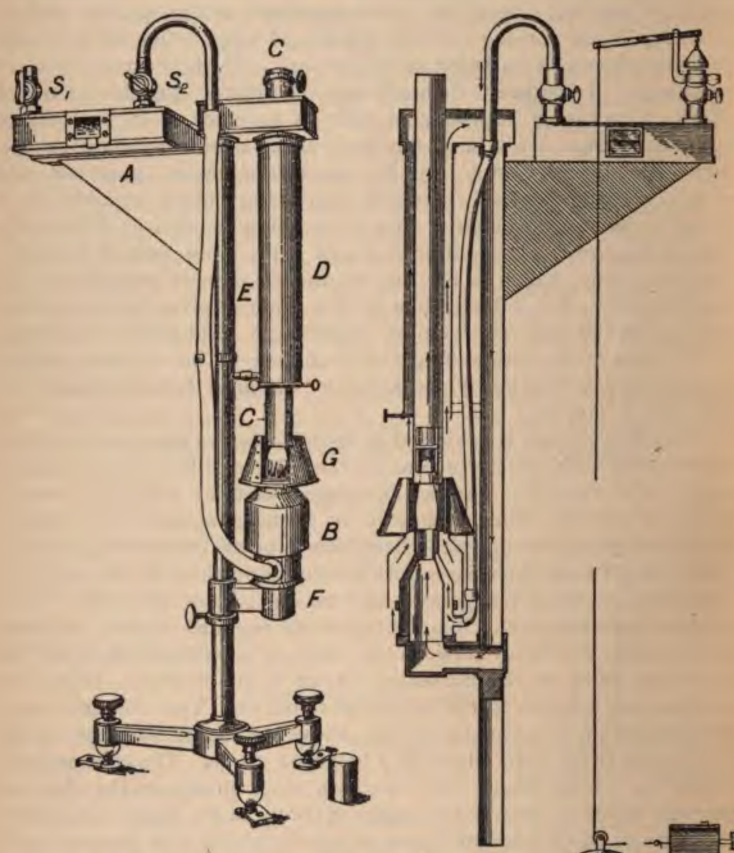


FIG. 1.—VERNON HARCOURT PENTANE TEN-CANDLE LAMP.[1]

chimney is surrounded by a larger brass tube, in which the air is warmed by the chimney, and so tends to rise. This makes a current which, descending through another tube, supplies air to the centre of the steatite ring. No glass chimney is required, and no exterior means have to be employed to drive the pentane vapour through the burner.

[1] The author is indebted for permission to make use of the block of Fig. 1 to the editor of the *Journal of Gas Lighting*, and acknowledgment is here gladly rendered for the courtesy.

"Fig. 1 shows the general appearance of the lamp. The saturator *A* is at starting about two-thirds full with pentane. It should be replenished from time to time, so that the height of liquid as seen against the windows may not fall below one-eighth of an inch. The saturator *A* is connected with the burner *B* by means of a piece of wide india-rubber tube. The rate of flow of the gas can be regulated by the stopcock S_2, or by checking the ingress of air at S_1. For this latter purpose, a metal cone, acting as a damper, is suspended by its apex from one end of a lever to the other end of which is attached a thread for moving the cone up or down. The lever is supported by an upright arm clamped to the upper end of the stopcock immediately beneath the cone. From the top of the lamp the thread descends to a small pulley on the table, and thence passes horizontally to the end of a screw moving in a small block, by turning which the gas examiner can regulate the lamp without leaving his seat. It is best so to turn the stopcock S_2 as to allow the flame to be definitely too high, but not to turn it full on before letting down the regulating cone to its working position. Both stopcocks should be turned off when the lamp is not alight.

"The chimney tube *C C* should be turned so that no light passing through the mica window near its base can fall upon the photoped. The lower end of this tube should, when the lamp is cold, be set 47 millimetres above the steatite ring-burner. A cylindrical boxwood gauge, 47 millimetres in length and 32 in diameter, is provided with the lamp to facilitate this adjustment. The exterior tube *D* communicates with the interior of the ring-burner by means of the connecting box above the tube *E*, and the bracket *F*, on which the burner *B* is supported (see sectional diagram, Fig. 1). A conical shade *G* is provided. This should be placed so that the whole surface of the flame beneath the tube *C* may be seen at the photoped through the opening.

"The lamp should be adjusted by its levelling screws so that the tube *E*, as tested with a plumb-line, is vertical, and so that the upper surface of the steatite burner is 353 millimetres from the table. A gauge is provided to facilitate this latter measurement. The tube *C* is brought centrally over the burner by means of the three adjusting screws at the base of the tube *D*. This adjustment is facilitated by means of the boxwood gauge.

"When the lamp is in use, the stopcocks are to be regulated so that the tip of the flame is about halfway between the bottom of the mica window and the cross-bar. A variation of a quarter of an inch either way has no material influence upon the light of the flame. The saturator *A* should be placed upon the bracket as far from the central column as the stop at the end will allow. If it is found, after the lamp has been lighted for a quarter of an hour, that the tendency of the flame is to become lower, the saturator may be placed a little nearer the central column.

"To prevent a gradual accumulation of dust in either the burner or the air-passage, a small cover of the size of the top of *B* and shaped like the lid of a pill box, should be kept upon the lamp when not in use."

This latest pattern of self-contained Pentane lamp is altogether superior as a standard to the 1-candle-power Pentane lamp with a wick, which was brought out some years ago to meet the requirements of a working standard.

For comparison with glow-lamps, a 10 c.p. standard is a more convenient unit than 1 c.p., and moreover, the earlier form of 1 c.p. lamp with a wick was more trouble to start in action and had other defects, which are absent in the 10 c.p. standard.

An important point in connection with this 10-candle Standard is that it requires and has no glass chimney. Other Pentane lamps have been produced in which a glass chimney is employed, but this feature always introduces an element of uncertainty into the working of any standard of light. Moreover, the light from the top of the flame is cut off by the metal chimney, and this probably contributes to prevent the light emitted being influenced by normal variations of atmospheric pressure so much as is the case with open flame lamps. The light is, however, affected by the presence of water vapour and carbon dioxide in the air, as in the case of all other flame standards.

The third flame standard, which has come into very general use, and is especially popular in Germany, no doubt on account of its German origin, is the so-called Hefner Lamp, which was introduced by Herr von Hefner-Alteneck in 1884.[1] This well-known lamp consists of a small metal body containing the combustible, and from out of it a metal tube made of German silver, containing the wick, rises. The tube is 8 mm. inside diameter, and 8·3 mm. outside diameter, and 25 mm. high. The wick is formed of strands of cotton yarn. Separate threads to the number of 15 or 20 are laid together straight, not twisted, until the size of wick is sufficient to fill up the tube without squeezing. The exact number of strands is not of great consequence, and only affects the height of the flame. By means of a simple rack mechanism the wick is moved up and down so as to alter the flame height, and by means of a small rod fixed at the top of the lamp carrying two metal sights, the flame can be adjusted to be exactly of the standard height of 40 mm.

The material burned in the lamp is Amyl Acetate $C_7H_{14}O_2$. The quantity of the combustible in the lamp does not matter as long as all the lower ends of the wick are well immersed. The wick should be trimmed square at the top of the tube, and after filling the lamp it should be allowed to burn ten minutes before adjusting the flame and making the measurement.

It was claimed by the inventor that the absolute purity of the Amyl Acetate was not of very great importance; but this has been lately denied, others asserting that it is essential to use chemically pure Amyl Acetate.

The lamp is used without a chimney, and as the flame is very lambent or mobile, it must be carefully protected from draughts. The luminous intensity of this flame is less than that of a British Standard candle. Measures of the ratio, however, made by different observers do not agree very well. Table I. (p. 129) is taken from a preliminary report of a Sub-committee of the American Institute of Electrical Engineers on Standards of Light, issued in 1896.[2]

The variation in the value of the ratio is partly due to the uncertain value of the British Parliamentary Candle, and partly to personal errors, but also to the different methods of comparison adopted, affecting the ratio in consequence of the difference in the quality of the two lights

[1] *Elektrotechnische Zeitschrift*, vol. 3, p. 445, and vol. 5, p. 20.
[2] See *Transactions of the Amer. Inst. of Elec. Eng.*, vol. 13, 1896.

TABLE I.

Observer.	Ratio of the Hefner Unit to the British Candle.
SHARP. From observations against Standard Candles reduced for rate of burning	0·872
SHARP. From observations against Standard Candles reduced for flame height	0·892
SHARP AND TURNBULL. From observations with the bolometer and candles	0·98
VIOLLE	0·98
REICHSANSTALT INVESTIGATIONS. Mean value	0·876
NETHERLAND PHOTOMETRY COMMISSION	0·921
S. SCHIELE. Mean value	0·881

compared. The most probable value appears to be 0·88.[1] Hence we may reasonably assume that luminous intensities expressed in Hefner units have to be multiplied by 0·88 to reduce them to their equivalent in British Standard Candles.

The chief objection that has always been raised (in countries other than Germany) to the use of the Hefner Lamp as a standard, is the reddish character of the light. In this respect it compares very unfavourably with the Harcourt 10-candle Pentane Lamp, the light of which is comparable in quality with that of a glow-lamp working at about 3 watts per candle-power. In other words, the Pentane flame is at a temperature nearer to that of the glow-lamp filament when in use. The employment of the Hefner Lamp as a means of standardising glow lamps when used at the ordinary efficiencies, gives rise to the difficulties of heterochromatic photometry, to which allusion will be made presently. Its use in arc-lamp photometry is out of the question. This lamp has been very carefully investigated by Liebenthal.[2] He studied the effect of water vapour, and carbon dioxide in the atmosphere, on the luminous intensity of the lamp. If w represents the volume of water vapour in litres per cubic metre of dry air, then the light (L) of the Hefner Lamp is expressed by the following linear function :—

$$L = 1{\cdot}049\,(1 - 0{\cdot}0053\,w).$$

The formula holds good between 3 and 18 litres of water vapour per cubic metre. The light of the Hefner Lamp decreases, therefore, about 0·5 per cent. per litre of water vapour, and has a value equal to unity, when 8·8 litres of water vapour per cubic metre of dry air are present in the atmosphere, according to the regulations of the Berlin Reichsanstalt.

Taking the average variations of moisture in the air from month to month, we find that this implies a variation of about 4 per cent. in the light between the wet and dry seasons of the year.

Again, if c represents the quantity of carbon dioxide present in the

[1] This is the value taken in a Specification for the supply of Glow Lamps, issued by the General Post Office. (See a paper by Sir W. H. Preece, F.R.S., on "Electric Glow-Lamp Tests," *Proc. Brit. Assoc.*, Liverpool, 1896, or *Electrician*, vol. 37, p. 738, 1896.)

[2] *Physical Society Abstracts*, vol. 1, abs. 501 ; *Elektrotechnische Zeitschrift*, 1895, vol. 16, p. 655 ; also *Zeitschrift für Instrumentenk.*, vol. 15, p. 157, 1895.

atmosphere in litres per cubic metre, then the luminous intensity L is expressed by the following formula :—

$$L = 1\cdot012\,(1 - 0\cdot0071\,c).$$

Also slight variations in the height of the flame have a great influence on the luminous intensity. If h is the height of the flame in millimetres, then the luminous intensity L is expressed by the following linear functions :—

$$L = (1 + 0\cdot025\,(h - 40)\,),$$
$$L = (1 - 0\cdot030\,(40 - h)\,),$$

according as h is above or below 40 mm. A change of 1 mm. in the height of the flame creates, therefore, a 3 per cent. change in the light.

Finally, variations in atmospheric pressure affect the light given by the lamp. Between 735 mm. and 775 mm. barometric pressure the light variation may be expressed by the following formula :—

$$\Delta L = 0\cdot00011\,(H - 760).$$

Where ΔL is the change in the value of the light, corresponding to a barometric height of H mm. This represents a variation of 0·1 per cent. for 10 mm.

Unless all these corrections are applied, the luminous intensity of the Hefner Lamp is uncertain within limits greater than those which can easily be determined photometrically.

The effect of carbon dioxide in the atmosphere on the luminosity is important. A change of 1 litre per cubic metre of air—that is to say, a variation of 1 part in 1,000—affects the intensity of the light 0·7 per cent. Hence it is quite clear that in badly ventilated rooms or rooms where many people are gathered together, carbon dioxide will be present to an extent which materially influences the light of the lamp.[1]

Turning, then, next to the subject of Incandescence Standards of Light, we may say that the only practical standards of this description which have been evolved, are those in which either platinum or carbon heated to a high temperature is employed. The platinum standard, suggested by M. Violle, was adopted as an International Standard at the Paris Congress of Electricians in 1884. M. Violle proposed in 1881 to define the unit of light as the light radiated normally from one square centimetre of platinum at its melting-point. The International Congress of Electricians in 1889 adopted the proposal that the practical unit of light should be one-twentieth part of the Violle Platinum unit. This sub-division was called the *bougie decimale*, this last term being the name for the tenth part of the carcel; the platinum unit having been found by M. Violle to be nearly equal to two carcels. Objections have been raised to the platinum unit on several grounds. In the first place, a very large mass of expensive metal is necessary, and the prac-

[1] For a series of curves showing the variation of the Hefner Lamp with barometric pressure and moisture, see the *Electrical Review*, vol. 42, p. 759, 1898.

tical difficulties in carrying out the photometric comparison with secondary standards were found to be considerable. An attempt was made at the Reichsanstalt in Berlin to reproduce the Violle Standard, but apparently with no very great success, and the British Association Committee on the Standards of Light in their Report, presented in 1888, stated that they consider that this standard was not a practical standard of light, although they were prepared to accept it as the definition of a unit. The meaning of this decision is not very clear. Since that date, however, a long research has been carried on at the Davy-Faraday Laboratory in 1899 by Mr. J. E. Petavel, who made the Violle unit the subject of a careful investigation.[1] The first operation is to melt a large mass of pure platinum, by means of the oxy-hydrogen blow-pipe, in a lime crucible. Mr. Petavel came to the conclusion that the essential conditions of success for the reproduction, by the use of molten platinum, of a constant standard of light are that :—

1. The platinum must be chemically pure.
2. The mass of it should not be less than 500 grams.
3. The crucible must be made of pure lime.
4. The hydrogen burned must contain no hydrocarbons.
5. The gases should be burned in the ratio of 4 volumes of hydrogen to 3 of oxygen.

The process of producing the unit of luminous intensity by the platinum standard consists in melting this mass of platinum under the above conditions. A water-cooled diaphragm screen is then placed over the molten metal, having in it an aperture one square centimetre in area. The light from the molten platinum is reflected to a photometer by a mirror, and the metal is then allowed to solidify. The temperature of the metal falls to the freezing point, and then remains practically constant until the solidification is completed. During this time of constant temperature, the light emitted from the selected area is also practically constant. Full details of the operations are given in Mr. Petavel's paper.

His inference from the whole of his work is that when carried out with the stated precautions, the probable variation in the light emitted by molten platinum under the standard conditions is not above 1 per cent., and he considers that with more perfect apparatus and with certain improvements the accuracy of this standard would be increased.

This investigation, therefore, seems to have rescued the Platinum Standard from some undeserved condemnation. In a similar manner, the platinum resistance-thermometer was at first underrated in value, until it was restored by Professor Callendar to its present position of utility. We have grounds, therefore, for the belief that the Platinum Standard in some perfected form may yet prove to be the best ultimate standard of luminous intensity. Apart from the not insuperable difficulties of its employment, it realises in a very perfect form the condi-

[1] See *Proc. Roy. Soc., vol. 65, p. 649.* J. E. Petavel, "An Experimental *Research on some Standards of Light.*"

tions which are necessary for such a standard, namely, a perfectly pure material maintained at an absolutely constant temperature in a definite condition, the luminous radiation from a unit of area of it then constituting the unit of luminous intensity. The Violle Platinum standard has the additional recommendation that it is not only a unit of light but also a unit of brightness. It is, however, a standard of such a nature that it is not likely to be set up anywhere except at the National Physical Laboratory. It is unnecessary to recapitulate here all the details of the various attempts that have been made to replace the Violle Platinum Standard and obtain an Incandescent Platinum Standard which could be more easily employed as a Working Standard. The most promising of these seemed at one time to be the method suggested by Lummer and Kurlbaum.[1] Briefly, their mode of defining the unit of luminous intensity was as follows :—

It was to be the light emitted from a square centimetre of solid platinum when brought by an electric current to such a temperature that 10 per cent. of its radiation, as measured by a bolometer, could pass through a layer of water two centimetres in thickness contained in a cell with quartz sides. For the details of this experiment the original paper may be consulted. The apparatus was established in the Reichsanstalt at Berlin, and is used at present as a standard of reference for Hefner Lamps.[2]

The only attempt to repeat this work in England has been (so the writer believes) made by Mr. J. E. Petavel, who set up the apparatus in the Davy-Faraday Laboratory (see *Proc. Roy. Soc.*, vol. 65, p. 478). His conclusion, however, was that the adjustments were very difficult, and in addition the spectral quality of the light not satisfactory as a standard, being much less white than that of the Violle Platinum Standard. Hence he gives it as his opinion that the Lummer-Kurlbaum Standard, in spite of the preference shown for it in Berlin, does not possess the qualities required in a primary Standard of Light.

The consideration of the possibility of using carbon filament lamps as standards of light has been very much before the mind of the author during the eighteen years that he has acted as Scientific Adviser to the Edison and Swan United Electric Light Company, Limited.

The difficulties with flame standards which arise from variations in atmospheric pressure and moisture, and from the contamination of the air in badly ventilated rooms by carbon dioxide, rendered it desirable to endeavour to devise a simple working standard which is independent of atmospheric composition. Hence, many years ago the writer's attention was called to the question of the use of the electric glow-lamp as a standard of luminous intensity.

The first objection that of course arises is that the decay in light-giving power of an ordinary carbon filament lamp, even when worked at a moderate efficiency, renders it perfectly valueless as a standard. A carbon filament lamp alters in light-giving power when used at constant voltage, for three reasons, viz., by—

[1] Lummer and Kurlbaum, *Elektrotechnische Zeitschrift*, vol. 20 (1894), p. 474.

[2] *The Reichsanstalt Unit of Light*, by Lummer and Kurlbaum, *Electrician*, *vol. 34, p. 37* and *p. 77*.

(1) Changes in electric resistance of the filament.
(2) Changes in the nature of the surface of the filament.
(3) The deposit of carbon upon the interior of the bulb.

It is well known that the candle-power of new glow-lamps of the majority of types increases for a short time after they have been put into use. This is due to a decrease in the resistance of the filament. The filament becomes more consolidated and probably denser, and therefore decreases in resistance. If, however, a good filament is run in a lamp at normal, or slightly above normal, voltage for fifty

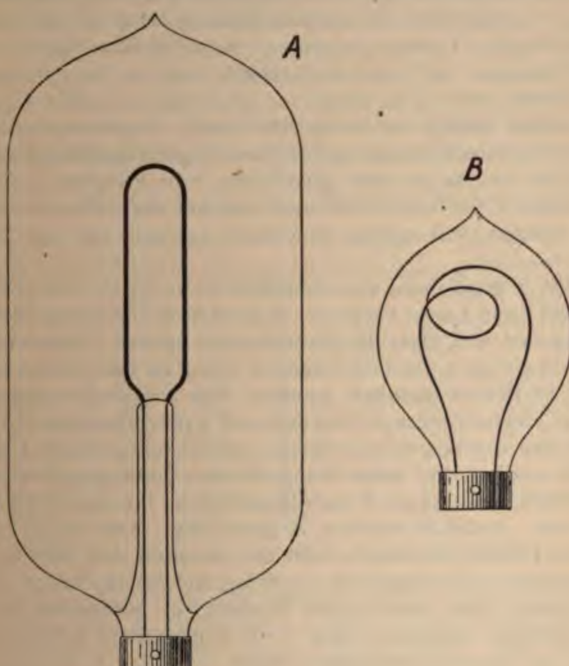


FIG. 2.

A. FLEMING-EDISWAN STANDARD GLOW-LAMP.
B. ORDINARY EDISWAN 16-C.P. GLOW-LAMP.

hours or so, it reaches a condition in which a small further use will not much alter it. By that time, however, the glass bulb is somewhat blackened, and the lamp will have lost candle-power. Experiments made by the writer several years ago, showed, however, that if a filament which has been so "aged" is removed from the old bulb, and put into a new clean bulb, the candle-power will again be brought back to a value not far from its original value. It therefore occurred to the writer to prevent the blackening of a glow-lamp by the following means: *The blackening is caused* by the projection of carbon from the

filament, and hence other things being equal proceeds most rapidly in small bulbs, because the carbon molecules then most easily reach the glass. Suppose, however, that a filament is mounted in a very large bulb, the radius of which is much greater than the mean free path of the molecules at the pressure of the residual air. The chances of a molecule of carbon getting on to the glass are much reduced. Accordingly, about six years ago, the author requested Mr. E. Gimingham, Superintendent of the Edison and Swan Electric Light Factory, to mount some 16-c.p. filaments in very large bulbs, and it was found that these lamps could be run for long periods without any of the usual blackening or loss of light. This observation led, therefore, to the following method of constructing a special form of carbon filament lamp to be used as a standard of light. Carbon filaments of the old Edison horseshoe shape are well selected and carefully treated, and are then mounted in ordinary bulbs, and run as lamps for some time, at about 5 per cent. above marked voltage for about fifty hours. Those filaments which then show no defects are cut out of these slightly blackened bulbs and mounted in very large clear glass bulbs, 6 or 8 inches in diameter. These lamps, if not very much used, and not worked above a certain marked voltage, will remain practically constant for any length of time.

In 1896 a good many experiments were made on this plan at the Edison and Swan Lamp Factory.[1] A number of these large bulb lamps were prepared, and carefully photometered against a pentane air-gas standard as giving us the best means of fixing at that time a standard equal to 10 British Standard candles. The working voltage of the lamps was carefully measured by means of a potentiometer and a Clark cell, and the candle-power, voltage, and current recorded on each lamp, the candle-power taken being that in a horizontal direction and perpendicular to the plane of the filament when the axis of the filament was vertical. A certain number of these lamps were set on one side, and called Primary Standards, with the intention that they should be used only very occasionally for verifying the candle-power of others of the lamps, which were called Working, or Secondary Standards. These secondary standards were to be employed to set the working lamps in the various photometer rooms.

A similar set of primary and secondary lamps of this description was made for the Pender Laboratory, University College, London. These lamps have been in use for several years in the Edison and Swan Company's Factory at Ponder's End, and at the Pender Laboratory, and have been found to be very convenient. In the month of March, 1902, with the kind co-operation of Mr. E. Gimingham, some experimants were made at the Edison-Swan Factory, Ponder's End, to ascertain how far any difference might have arisen in six years between

[1] The author desires to take this opportunity of gratefully acknowledging the kind encouragement and valuable support he has continually received from Mr. J. W. Swan, F.R.S., in this and many other similar investigations during the whole period of his connection as Scientific Adviser with the Edison and Swan United Electric Light Company, Limited. In addition, the author has pleasure in referring to the assistance rendered by Mr. E. Gimingham in working out this form of Standard Ediswan Glow Lamp.

the Pentane air-gas standard used there, and certain of these standard carbon filament lamps. The lamps were accordingly checked in a photometer room, using a Lummer-Brodhun photometer to measure the light, and a Crompton potentiometer and Clark cell to measure the current and voltage. In order to eliminate the personal error, three or more observers were admitted to the photometric gallery. This somewhat unusual proceeding, however, and the want of attention on one occasion to the ventilation, resulted in revealing an apparent discrepancy between the candle-power of the lamps as measured in terms of the Pentane flame in 1896, and those made in March, 1902.[1] The following table shows the results of these first observations, the three observers being denoted by the figures (i.), (ii.), and (iii.) :—

TABLE II.

Comparisons between the Pentane Air-gas Standard and Large Bulb Standard Incandescence Electric Lamps.

Readings taken March 8, 1902, at the Edison-Swan Factory, Ponder's End

Mark on Standard Glow Lamp.	Working Volts on Lamp.	Candle-power by Pentane Standard read by Three Observers.				Candle-power of Glow Lamp as determined previously.
		(i.)	(ii.)	(iii.)	Mean c.p.	
Ediswan R_2 ...	99·1	14·6	14·4	—	14·5	14·0 in Feb., 1902
Ediswan S_4 ...	96·2	10·4	—	10·4	10·4	10·0 in 1896
Ediswan S_3 ...	96·1	10·4	10·6	10·6	10·5	10·0 in Jan., 1902
Pender I..........	96·0	14·8	14·6	—	14·7	14·3 in 1896
Pender II.	96·0	16·9	16·8	16·5	16·7	16·4 in 1896
Pender III.......	96·0	14·5	14·2	14·2	14·3	12·75 in 1896
Test repeated...	"	13·75	13·75	—	13·75	
Ditto	"	14·0	13·9	—	13·95	

If we were entitled to take for granted that the Pentane Standard had remained unaltered, the above table would seem to show a falling off in the candle-power of the glow-lamps in the six years. This, however, is not a valid conclusion. The greater difference between the 1896 and 1902 measurements in the case of the lamp Pender Standard III. showed that probably all was not right on this occasion with the Pentane lamp. This, in fact, was the case. The presence of an unusual number of persons (four) in the photometer room vitiated the air towards the end of the time of observations. The doors were therefore all thrown open for forty minutes, and after the air in the room had been thoroughly renewed the measurements were repeated as below :—

[1] The observations in 1896 were made with great care by Mr. J. T Morris, at one time private assistant to the author, but now Lecturer on Electrical Engineering in the East London Technical College. The observations in 1902 were made under the direction of Mr. W. C. Clinton, B.Sc., Demonstrator in the Pender Laboratory, and to whom the author is indebted for valued and willing assistance in the experiments here described, as well as many others related to this investigation.

TABLE III.

Second Set of Readings taken with Photometer Room well ventilated.

Mark on Standard Glow Lamp.	Working Volts on Lamp.	Candle-power by Pentane Standard read by Three Observers.				Candle-power of Glow Lamp as determined previously.
		(i.)	(ii.)	(iii.)	Mean c.p.	
Pender I.	96·0	14·4	14·25	14·0	14·22	14·25 in 1896
Pender II.	96·0	16·5	16·7	16·0	16·4	16·4 in 1896
Pender III.......	96·0	12·75	13·0	12·5	12·75	12·75 in 1896
Pender IV.	96·0	14·1	—	13·9	14·0	14·5 in 1896
Pender V.	96·0	15·5	15·25	15·0	15·4	15·55 in 1896
Pender VI.	96·0	11·9	11·7	11·65	11·75	11·5 in 1896
Ediswan S_4 ...	96·2	10·1	9·8	9·8	9·81	10·0 in 1901
Repeated......	"	9·7	9·65	9·8		

During these tests the temperature of the Clark cell rose from 16° C. to 20° C. This alone would imply a possible error in voltage of 0·36 per cent., and an uncertainty therefore in candle-power of 1·8 per cent. The results of the measurement are, however, to show that in six years, during which these glow-lamp standards have been much used, the set belonging to the Pender Laboratory have probably remained as constant in light-giving power at the same voltage as the Pentane Standard. Since a standard glow-lamp of this form, used as described in the next section of the paper, is only in a state of incandescence for a few minutes at a time during each test, the use of such a lamp in many hundreds of tests only amounts in all to a few hours' burning. It is clear, therefore, that if the filament has been brought into a condition in which it has passed the initial variable stage during which changes may take place in it, and if after that time it is only used for exceedingly short periods of time, and in a large bulb as described, it becomes a means of preserving a standard of light with great constancy.

The differences in the figures in the above Table III. between the lamp Pender IV. and Ediswan S_4, and Pender IV. and the Pentane Standard, must not be attributed wholly to changes in the standard glow-lamp. In the case of the test made with the standard lamp Pender IV. it will be seen that the candle-powers were only read by two out of the three observers. With this exception, none of the lamps which have been in use in the Pender Laboratory for six years now appeared to differ from the Pentane Standard by more than 2 per cent., and in the majority of cases there is no sensible difference, and, as observed above, the whole of this difference, where it appears, must not be set down to changes in the incandescent lamp. In fact, these large bulb glow-lamp standards were easily able to detect a temporary variation in the Pentane Standard due to inattention to the ventilation of the room.

This error was not small. At the end of the first set of tests the atmosphere had become vitiated to such a degree that, partly for this reason and partly perhaps from errors in adjustment of flame

height, the flame standard had fallen off in illuminating power by 8 to 10 per cent., and hence made an incandescent lamp of which the real candle-power was 12·75 appear to be about 14·3. The operation was not, in fact, a test of the incandescence lamps by the Pentane, but a test of the latter by the former.

The experience, however, gained by the writer in the last six years, and also at the Edison Swan Lamp Factory, justifies the expression of opinion that these large bulb standard carbon filament lamps form a very convenient and accurate means of preserving a standard of light when proper precautions are taken to set them to a marked voltage by means of a potentiometer and Clark or Weston standard cell.

One other suggested incandescence standard must be briefly mentioned. It was proposed by Mr. Swinburne, Professor S. P. Thompson, M. Blondel, and others, that the light from one square millimetre of the crater of the arc-lamp should be taken as a standard of luminous intensity. Measurements of the intrinsic brilliancy of the crater by different observers do not agree very well, and it cannot be said that the experimental work done so far holds out a promise that this source of light will fulfil all the requirements of a standard.

Measurements of the intrinsic brightness of the arc crater made by Mr. Trotter, M. Blondel (in 1893), and Mr. Petavel (in 1900) gave values for this constant respectively of 170, 158, and 147 candles per square millimetre. The actual incandescent area which forms the crater is, however, very small, and, according to a discovery made by Mr. Trotter it very often exhibits a rapid rotatory movement, so that it is not so simple, physically speaking, as a surface of molten platinum. The intrinsic brilliancy of the crater is so great, and the quality of its light is so different from that of most secondary standards, that it is a matter of greater difficulty to compare this arc standard with a secondary standard than is the case with the Violle Platinum Standard. Hence for all these reasons an incandescent platinum standard will probably be preferred.

The conclusion, therefore, which may be drawn from the preceding facts is that at the present moment there are five sources of light which can in all probability be regarded as sufficiently constant to enable them to be used for reproducing a standard of luminous intensity with a degree of accuracy approximating to 1 per cent. or less. Two of these may be called primary or reference standards, and three working standards. The Primary Standards are :—

1. The Violle Platinum Incandescent Standard.
2. The Vernon-Harcourt Pentane 1-candle Flame Standard ;

and as practical Working Standards :—

3. The Hefner or Amyl Acetate Lamp.
4. The Vernon-Harcourt Self-contained 10-candle Pentane Lamp.
5. The Fleming-Ediswan Large Bulb Incandescence Electric Lamps.

The first two Reference Standards can be set up at a National Standardising Laboratory, or a Government Testing Laboratory, and

can be relied upon to preserve a selected standard or unit of light with an accuracy which is comparable with that of photometric measurements generally.

Of the three Working Standards, the Amyl Acetate Lamp is decidedly inferior to the other two in the quality of its light, and difficulties arise in using it even to standardise glow-lamps, whilst it is quite unsuitable for use with arc-lamps. Moreover, experience shows that a 1-candle standard is not so generally useful as a 10-candle.

One objection which has been raised to the employment of the Violle standard as a primary standard is that it involves the use of a mirror to reflect the vertical ray from the molten platinum into the photometer, being used to compare it with a secondary flame standard, and hence there is possibility of error produced by the slight uncertainty attaching to the co-efficient of the reflection of the mirror. The defect could be obviated by employing a photometer placed vertically over the molten platinum to compare the emitted light from it with a large bulb glow-lamp standard made on the author's plan, and this again could be compared with any required flame standard, the ray from which must necessarily be horizontal.

Other objections have been raised to the employment of the Violle standard as a practical primary reference standard, such as the difficulties likely to arise from the column of hot air ascending from the platinum.[1] A further investigation of this standard is therefore much to be desired, and it seems a piece of work that might very suitably be undertaken in the National Physical Laboratory, where it is to be hoped a Primary Reference Standard of light may before long be established.

II. Photometric Processes.

It would cause the present paper to greatly exceed reasonable limits in length, if any attempt were made to discuss the whole of the photometric processes which have been devised. It is certainly not necessary to repeat here information which can be obtained from ordinary treatises on photometry. Generally speaking, a photometric measurement consists in comparing together the brightness of two white surfaces, one illuminated solely by the light under test, and the other by a standard light, and adjusting the distance of the lights until an equality in brightness or illumination is secured. A typical and simple form of photometer, therefore, is the Ritchie wedge, in which two adjacent sides of a white prism inclined at equal angles to the incident rays serve as the two surfaces which are differently illuminated. Whatever may be the exact nature of the arrangement for creating these two contiguous surfaces illuminated by different sources of light, it appears to be an essential condition for sensitiveness that they shall not be separated by any dark or bright space not illuminated wholly by one light or the other. Thus, for instance, the accuracy with which measurements can be made by the Ritchie wedge is greatly decreased

[1] *This source* of error was suggested to the author in conversation by Mr. *A. G. Vernon Harcourt.*

if the edge of the wedge is blunt. If the eye has to travel far in going from one surface to the other, then the power to make a correct judgment as to the equality in the brightness of the two surfaces is greatly diminished. Hence, whatever form a photometer may take, it must be in one in which this accurate juxtaposition of the two surfaces to be compared can be secured. Another condition is, that the illuminated surfaces must be perfectly white. There is a vast difference between surfaces in regard to whiteness, which are all called white. Paper, cardboard, and newly fallen snow, look very different when illuminated by the same source of light under the same circumstances. A suitable white surface for photometry can be obtained by compressing magnesium carbonate or barium sulphate into slabs. In the Lummer-Brodhun

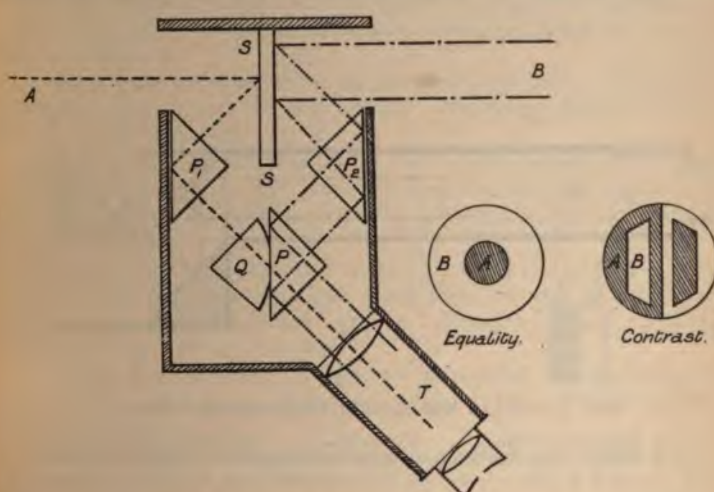


FIG. 3.—LUMMER-BRODHUN PHOTOMETER.

S. Magnesia Screen. P_1 P_2. Totally reflecting Prisms.
Q P. Lummer-Brodhun Prism. T. Telescope.

photometer, such a white magnesia slab is illuminated on its two opposite sides by the two lights to be compared (see Fig. 3). By the means of two totally reflecting prisms, P_1, P_2, the diffused light from the two sides is sent through a compound glass prism, P Q, consisting of two right-angle prisms placed base to base. One of these prisms has portions of its hypothenuse surface removed by sand-blasting, so as to be at a lower level than the rest, and the two right-angle prisms have their hypothenuse surfaces placed together, being faced to come into optical contact where they touch.

When such a prism is viewed by means of a telescope and an eye-piece in the proper position, we see the field of view divided into two parts, one portion of which is illuminated by the diffused light scattered from one side of the magnesia slab, and the other side by light scattered from the other. By adjusting the distances of the lights,

the brightness of these two portions of the field of view can be made to agree. If the lights are heterochromatic, then these two portions of the field of view have different colours as well as different brightness. and the observer has to make a judgment when the two patches agree in brightness without regard to their difference in tint.[1] The author has devised a modification of the Lummer-Brodhun photometer, in which the two lights, A and B, to be compared are placed one in front and one at the side of the prism box. This form has some advantages for arc-lamp testing (see Fig. 4).

Some considerations affecting heterochromatic photometry are discussed below, but meanwhile it may be said that there are various methods for reducing the distraction caused by this colour difference in the lights compared. In the old form of Bunsen grease-spot photo-

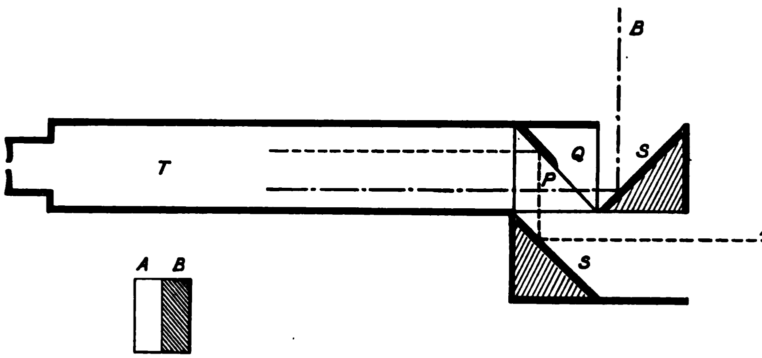


FIG. 4.—TOTAL REFLECTION PHOTOMETER (FLEMING).

P Q. Right-angle Prisms. T. Eye-tube. S S. White diffusing Screens.
A B. Field of View—the two parts illuminated respectively by light A and light B.

metry, the difficulty of making an accurate judgment as to the equality in brightness of the two surfaces to be compared, was considerable, but the difficulty was obviated to some extent in the form of disc known as the Star Disc, in which the simple grease spot of Bunsen was replaced by a sheet of tissue paper placed between two discs of thin white cardboard, in both of which a star-shaped opening had been punched out.

It is unnecessary here to attempt to make a complete classification of photometers. One which is sufficient for the present purposes is as follows :—

Photometers are divided into three principal classes :—

(A) *Intensity Photometers*, by means of which a comparison is made between the luminous intensity of two sources of light.

(B) *Illumination Photometers*, by means of which we measure the illumination in any locality in *candle-feet* or some similar units.

[1] *See Lummer* and Brodhun, *Zeitschrift für Instrumentenkunde*, vol. 9, *p. 23, 1889 ; also Ibid.*, p. 461, 1889 ; also Phil. Mag., vol. 49, p. 541, 1900.

(C) *Spectrophotometers*, in which selected rays from the spectra of two lights are compared in respect of luminous intensity.

The intensity photometers may be classified according to the method adopted for producing two adjacent surfaces or comparing the brightness of the two surfaces illuminated by the lights compared. Thus, we have photometers which operate by :—

(*a*) Equalising the illumination of two portions of a semi-transparent or opaque screen formed of paper, porcelain, or ground glass, the one portion illuminated by one light and the other by a standard, the equalisation being effected by moving the sources of light to various distances. *Examples:* Bouguer, Foucault, Harcourt, and Ritchie.

(*b*) Equalising the two shadows of a rod made by two lights moving to different distances with or without optical dispersion of one light by a lens. *Examples:* Lambert, commonly called the Rumford, Abney, Ayrton, and Perry.

(*c*) Equalising the illumination all over a screen, one portion of which is semi-transparent and the rest opaque. *Examples:* Bunsen (grease spot), Leeson and Dibdin (star disc).

(*d*) Equalising the illumination of two white surfaces inclined at equal or unequal angles, and placed in line between the lights to be compared. *Examples:* Ritchie, Bunsen and Roscoe, Trotter, Thompson and Starling.

(*e*) Equalising the illumination of two portions of the field of view of a telescope by bringing light from two sources to each part separately by total reflection in prisms. *Examples:* Swan, Lummer-Brodhun, Weber, Krüss, Fleming.

(*f*) Equalising two fields of light by weakening one by means of crossed polarising prisms. *Examples:* Arago, Zollner, Wild, Salomons, Pickering, Nichols.

(*g*) Equalising the illumination on two portions of a white surface by weakening one of the illuminations by interposing a rotating disc having a sector cut out of it which can be varied in magnitude. *Examples:* Fox-Talbot, Napoli, Guthrie, Abney.

(*h*) Equalising two fields of illumination by the interposition of an absorbing wedge. *Examples:* Pritchard, Sabine, and others.[1]

Probably by far the largest portion of photometry of late years has been conducted by means of the Bunsen grease-spot disc, or its various modifications. The Gas Referees have recently adopted a modified form of Foucault photometer which is called a *Photoped*. This consists of a small sheet of some transparent paper without watermark, which is fixed at the bottom of a short tube, having a diaphragm in it with a rectangular aperture. The diaphragm can be moved nearer or farther from the paper. When two lights are placed not quite close together, and at different distances, they throw upon the paper two patches of light due to the light passing through the aperture. By moving the

[1] For a discussion of Illumination Photometers and results obtained by them, the reader may with advantage consult a paper by Mr. A. P. Trotter, "On the Measurement of Illumination," *Proc. Inst. Civil Engineers*, vol. 110, 1892. A number of interesting photometers, such as the diffusion photometers of Joly and Elster, are not included in the above classification.

diaphragm, these patches of light can be made to touch. One of the lights can then be altered or moved until the illumination on the screen is uniform, and from their relative distances the relative illuminations are determined. The author's assistant, Mr. A. Blok, has found that a very effective diaphragm for this purpose can be made by using an ordinary gelatine photographic plate just as taken from a packet of unexposed negative plates.

For electric glow-lamp photometry the writer has found no photometer which is on the whole superior to the Contrast form of the Lummer-Brodhun photometer, which, when skilfully used, enables a difference of less than half per cent. in the luminous intensity of the two lights to be determined.[1]

In describing the arrangements which the writer's experience has shown to be the most advantageous for glow-lamp photometry, it will be well to say a few words first on the arrangement of the photometer and the photometer room. The general impression in the minds of many electricians is that any room or corner is good enough for a photometer. In numerous electrical laboratories or testing-rooms, a wooden shelf is put up with black velvet curtains in front, and a box at each end to hold the standard lamp and the lamp to be tested, whilst the Bunsen disc or other photometer slides on a graduated bar between the lamps. A photometer of this kind embodies almost every defect a photometer can have. It reproduces, often in an aggravated manner, the defects present in a form of photometer called the Evans closed photometer, long known to be unreliable. The chief source of error in it is that reflection of stray light from the neighbouring velvet or black wood surfaces causes the illumination on the photometer disc to vary *not* according to the law of the inverse square of the distance from the source of light. A photometer consisting of a long box or narrow shelf invariably allows a good deal of light to be reflected at an oblique incidence even from black velveteen curtains. The whole principle on which intensity photometry is based, is that no light must reach the photometer disc except that coming in straight lines from the two sources. Hence it is essential not to take this for granted in any particular instance, but to verify it. In the next place, such a closed photometer, if used with a flame standard of any kind, invariably gives erroneous measurements because of imperfect ventilation. If a flame standard is to be used at all, the greatest attention must be paid to the temperature and ventilation of the photometer room. For this purpose, it should be at least 8 feet wide, 8 or 9 feet high, and 20 feet in length. Fresh air should be drawn in from the outside by means of a fan, and circulated through the room, but with the avoidance of draughts. If this is not done, and two or three people are in the photometric room employing a flame standard, it will most certainly fall off in luminous

[1] The Lummer-Brodhun photometer is in reality a very superior form of Bunsen grease-spot photometer. The principle of employing total reflection at a prism surface to construct a photometer, was made use of by W. Swan, Professor of Natural Philosophy, University of St. Andrews. See a paper by Professor C. G. Knott, "On Swan's Prism Photometer," *Phil. Mag.*, vol. 49, Jan., 1900, and reply by Messrs. Lummer and Brodhun, *Phil. Mag.*, vol. 49, June, 1900.

intensity by a sensible percentage in a short time, owing to the accumulation of moisture and carbonic dioxide in the room. Also the temperature of the room should be kept uniform, especially if electrical measurements are to be made in it. The arrangements of a suitable photometric room for glow-lamp testing are as follows :—The room, being at least of the dimensions above stated, should be painted dead black in its interior, well ventilated as described, and kept at a constant temperature. Down the centre should run a wooden railway, consisting of a pair of beams on which can travel easily wooden slabs or tables holding the lamp to be tested, the standard and the photometer, the height being such as to bring the photometer telescope or tube to a level convenient for the eyes of ordinary persons when standing.

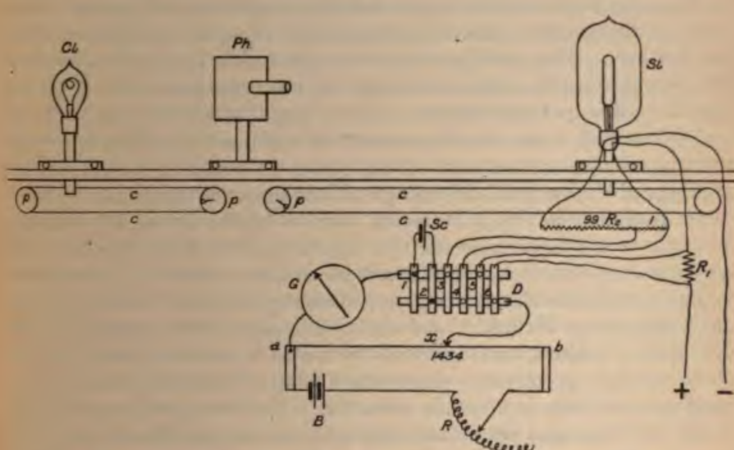


FIG. 5.—ARRANGEMENTS OF STANDARD PHOTOMETER BENCH AND POTENTIOMETER.

Cl. Comparison Lamp.
Ph. Lummer-Brodhun Photometer.
a b. Potentiometer Wire.
G. Galvanometer.
Sc. Standard Cell.
R_2. Voltage divided Resistance.
Sl. Standard Lamp.
p p. Pulleys. c c. Endless Cords.
B. Battery.
R. Rheostat.
R_1. Series Resistance.
D. Plug Switch.

The arrangements adopted after long experience in the Pender Electrical Laboratory at University College, London, and reproduced in the photometer rooms of the Edison and Swan Electric Lighting Company's Factory at Ponder's End, at the suggestion of the writer, are as follows :—The photometer employed is the Lummer-Brodhun photometer with contrast prism, and is kept fixed in one position on the railway (see Fig. 5). On the left-hand side, carried on a sliding table, is an incandescent lamp, which is called the *Comparison Lamp*, and by means of appropriate resistances this lamp can be adjusted accurately for voltage and therefore candle-power. The sliding table on the right-hand side is connected to an endless cord moved by a winch, so that the observer at the photometer can move the lamp

on this table to or from him. This slab carries a support for the incandescent lamp to be tested, so arranged that the lamp can be placed with its axis in any required direction, and also revolved on its axis by means of a small electric motor. Under the railway are placed the resistances and controlling handles for regulating the current and voltage of the lamp to be tested, and for this purpose, from the socket holding the above lamp, there come two pairs of leads, one the current leads, and the other the potential leads.

For measuring the electrical quantities, no instrument is so satisfactory as the direct-reading potentiometer. Out of a large experience, the writer can say that no ammeter or voltmeter yet made is sufficiently accurate for electric-lamp photometry. It was with this object that the writer introduced as far back as 1888 the direct-reading potentiometer, set by a Clark cell, which has been since brought into its present perfect form by Lieutenant-Colonel Crompton, and those of his firm who have assisted him. The modification which the author then introduced was that of adjusting the current through the potentiometer coils or wire, so that the fall of potential down a unit of length of the wire was equal to one-thousandth or one ten-thousandth of a volt as determined by comparison with a standard cell. Employing a Crompton potentiometer, readings of the current and voltage of incandescent lamps can be taken quite as quickly in the photometer room as by the use of any ordinary ammeter or voltmeter, and with an accuracy which is far greater. The only point to which attention need be drawn is that the temperature variation of the Clark cell being considerable, it is better to use a Weston cadmium cell, or Helmholtz calomel cell in place of the Clark cell. The operation of making measurements by means of the large bulb standard glow-lamps is as follows:—A standard lamp is selected giving, say, 16 candles at 96 volts in a certain direction. This standard lamp is placed in the testing socket with its axis upright, and set at a distance of 4 feet from the photometer disc. The distance of the comparison lamp is then varied until the photometric balance is obtained. The standard lamp is then removed from the testing socket and the lamp to be tested placed therein, and its distance varied until a photometric balance is again obtained. From the relative distances of the tested lamp and the standard, the luminous intensity of the former is determined in terms of the latter. The railway bar can, of course, be calibrated to show at once candle-power. It will be seen that this process is a form of *double weighing*. It eliminates the effect of any want of symmetry in the photometer itself. The exact candle-power of the comparison lamp does not matter, as long as it remains constant during the experiment.

In making a series of tests of incandescent lamps, it is desirable to check the setting of the comparison lamp by means of a large bulb standard, at intervals, just as the setting of the potentiometer is checked at intervals by means of the standard cell. In making photometric examinations of incandescent lamps, it is of course necessary to take the candle-power in different directions. In order to eliminate the variation in candle-power which exists in different horizontal directions when the axis of the tested lamp is vertical, a committee of the American Institute of Electrical Engineers recommended that the lamp under

test should be revolved on its vertical axis. This is not difficult with stiff filaments, but with the long high-voltage filaments now used there is a risk of breaking the filament, or forcing it against the bulb of the lamp, if the speed of revolution exceeds about two per second, and this is hardly sufficient to eliminate all flickering. In any case, the maximum horizontal and minimum horizontal candle-power should be taken, and also the candle-power in the direction of the axis of the lamp. For certain purposes, it may be necessary to take the mean spherical candle-power. The usual process is to be content with taking the maximum candle-power in a horizontal direction, but since by far the larger number of lamps are hung head downwards, in use this value alone does not give sufficient information as to the performance of the lamp, and the candle-power in the above-stated three directions should always be furnished. In the actual photometric measurements it is desirable to oscillate or move one of the lamps on a plan recommended by Sir W. de W. Abney. If the lamp under test is moved to and from the photometer in gradually diminishing arcs, it is easier to determine the exact position of balance than if this is not done. One advantage of the above described method is that the comparison lamp can be adjusted to work as nearly as possible at the same watts per candle as the lamps under test. This is especially desirable when using the Lummer-Brodhun photometer, as it is very sensitive to small differences in the spectral quality of lights compared.

Before proceeding to discuss the special difficulties connected with the photometry of arc-lamps, it may be well to describe the arrangement which the writer has found in practice to work well for determining the form of the polar curve of luminous intensity of arc-lamps.

Owing to the unsymmetrical distribution of light from an arc-lamp, it is more important than in the case of the incandescent lamp to determine the luminous intensity in different directions, and to set these out in the form of a polar curve, the radii of which represent to scale luminous intensity. Various devices have been used for this purpose, but a convenient arrangement is as follows :—On a suitable base is erected a wooden gallows about 9 feet high and 3 feet wide. From the top of this, the arc-lamp to be investigated is suspended. In the two uprights of the gallows are two openings through which pass brass tubes or hollow bearings to which are connected another rectangular frame, as shown in the diagram (see Fig. 6). The lamp is placed so that the arc A is exactly in line with the axis of these hollow trunnions. On the outside of one of the uprights is a circular scale of degrees, and the swinging frame carries a pointer, by means of which its angular position relatively to the horizon is determined. The swinging frame also carries three plane mirrors, I_1, I_2, I_3, which are set at angles of 45 degrees, and catch the ray from the arc-lamp, and reflect it down one of the hollow trunnions. The ray therefore emerges in the same direction, no matter what may be the angular position of the swinging frame. This frame can be so set as to catch a ray coming from the arc at any angle above or below the horizon. It is quite possible, by means of a standard incandescent lamp, to determine the total and constant percentage loss of light, by

the three mirrors at each of which the ray is reflected at an incide: of 45°, and hence to apply the necessary correction to the measu intensity of the selected ray. By employing a photometer an(standard glow-lamp, measurements can be made of the lumin intensity of the arc in any direction relatively to the horizontal pl:

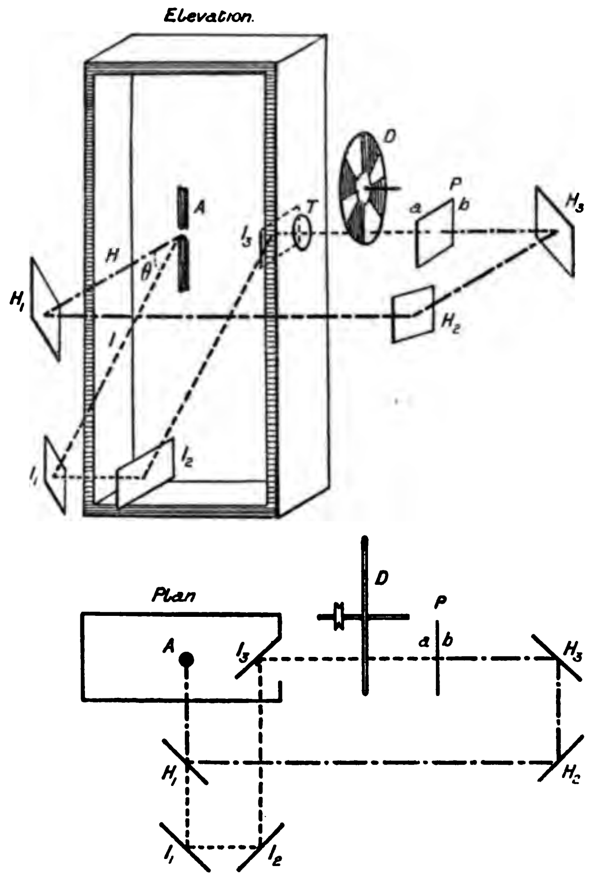


FIG. 6.—ARC-LAMP PHOTOMETER (FLEMING).

A. Arc Lamp. H_1 H_2 H_3 I_1 I_2 I_3. Mirrors.
P. Photometer Disc. D. Fox-Talbot Sector Disc.

through the arc. This direct measurement is often rendered diffi(because the electric arc shifts its position continually, and there therefore a periodic waxing and waning of the light in any directi(This difficulty, however, can be overcome by photometering the a against itself, or, in other words, comparing the luminous intensity of t *ray coming from* the arc in any direction with that of the ray comi

off in a horizontal direction. This is accomplished by fixing three other mirrors, H_1, H_2, H_3, to reflect round the ray coming in a horizontal direction from the arc, and make it coincide in direction with the three times reflected ray coming from off the arc at any angle above or below the horizon. In each case the ray suffers reflection at three mirrors placed at angles of 45 degrees, hence there is no difference in the loss by reflection, and both the rays are weakened in the same ratio.[1] We have then to determine the ratio of the intensities of these two rays. One way by which it can be achieved is by employing the device, much used by Sir W. de W. Abney, viz., a rotating metal

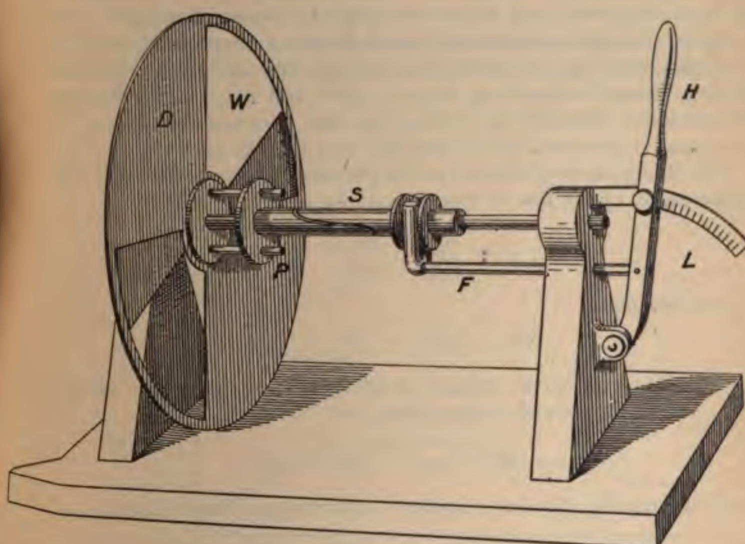


FIG. 7.—FOX-TALBOT-ABNEY VARIABLE APERTURE DISC.

disc having sectors cut out of it, the apertures in which can be more or less closed up whilst the disc is running[2] (see Fig. 7). If we interpose such a rotating disc with variable apertures in it, in the path of the brighter ray or in the paths of both rays we can weaken the stronger ray until the two are of the same intensity. By the use of a Foucault or Rumford photometer, and a pair of such rotating discs, we can accordingly determine the ratio of the luminous intensity of

[1] Many forms of arc-lamp photometer have been devised, such as that of M. Rousseau in which the ray is reflected from a mirror at various angles of incidence, but this requires a correction at every angle for loss by reflection.

[2] This method for weakening a ray of light in a known ratio was originated by Mr. Fox-Talbot, whose name is well known in connection with the development of photography. The use of a rotating disk with apertures which could be more or less closed, and through which the ray was sent, was described by him in the *Philosophical Magazine* for 1834, vol. 5, p. 331. See also Lord Rayleigh's article, "Wave Theory," *Encyclopædia Britannica*, 9th Ed.

the ray coming from the arc in any inclined direction to that emitted in a horizontal direction. The question whether the Fox-Talbot sector disc reduces the intensity of a transmitted ray in the proportion of the total open sectorial angle to 360° has often been raised. Experiments made in the Pender Laboratory for the author by Mr. W. C. Clinton support the affirmative conclusion generally accepted. An incandescence lamp was placed on a photometer bench and balanced against a Comparison lamp by means of a Lummer-Brodhun photometer. Between the photometer and the Comparison lamp a Fox-Talbot disc was interposed, and the balance between the two lamps was obtained for various illuminations by reducing the apertures of the rotating disc, and by moving one lamp at the same time to various distances. Thus the lamp being first placed at 80 inches from the photometer, a balance was found when the disc apertures were 97 per cent. open. The lamp was then moved respectively to 120″, 160″ and 204″, and the photometer balance obtained by closing the disc apertures successively to 44, 24, and 15 per cent. of full aperture reckoned as 100.

The illuminations produced on the photometer disc at these distances are therefore, by the law of inverse squares, respectively—

$$1 : \frac{64}{144} : \frac{64}{256} : \frac{64}{416};$$

or as percentages—

$$100 : 44{\cdot}4 : 25 : 15{\cdot}4;$$

and the above figures are almost precisely in the ratio of the aperture of the disc in the several experiments, viz.: —

$$97 : 44 : 24 : 15;$$

since on reducing the above figures to percentages they become—

$$100 : 45{\cdot}3 : 24{\cdot}75 : 15{\cdot}5;$$

and with the exception of the second, agree closely with the ratio deduced from the law of inverse squares.

A single observation, or rather the mean of a number of observations, taken against a standard glow-lamp will enable us to determine the mean absolute horizontal luminous intensity, and hence the polar curve of luminous intensity can be plotted out.

In taking the absolute luminous intensity of the arc, the most convenient standard to use is the glow-lamp of the large bulb pattern, which is worked at an efficiency of $2\frac{1}{2}$ watts per candle. There is no reason why manufacturers of arc-lamps should not furnish on demand the polar curve of luminous intensity, because such a curve at once enables us to determine the mean spherical candle-power, and also the illumination on the surface of a roadway, due to the arrangement of any number of such arc-lamps at known heights and distances. The geometrical constructions for doing this are tolerably well known, but it may perhaps be convenient to give them here.

Let A (see Fig. 8) be an arc placed on a post X A standing on a

roadway X Y. It is required to determine the illumination at any point P. Draw the line A P, and round A set off the polar curve of candle-power of the arc as determined experimentally. Let G E F be a semicircle just touching this polar curve. On the other side of the line X A and on the base F G describe a rectangle K F G H of which the side K F is equal to the maximum radius vector of the polar curve. Draw the horizontal line through A, draw a line B D vertically through B, and through Q where A P intersects the semicircle draw Q C horizontally and produce it to L, setting off a length C L equal to A B. At P set up P M perpendicular to X Y, and make P M equal to the quotient of B D divided by $(A\ P)^2$. This can be done at once by means of a slide rule.

Then if A B represent to any scale the luminous intensity of the arc in the direction A P, M P will represent the horizontal illumination on the roadway at P.

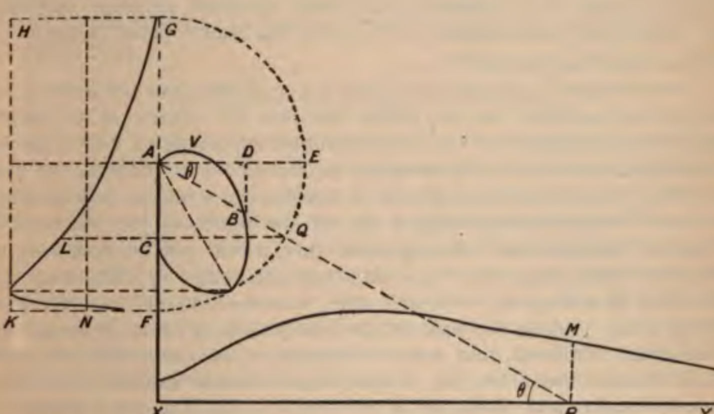


FIG. 8.—ARC-LAMP POLAR CURVE OF INTENSITY AND HORIZONTAL ILLUMINATION CURVE.

If other radii of the polar curve are drawn and the same construction followed out, then the extremities of all the lines similar to C L will define a curve G L F, called the Rousseau curve, which has the property that its mean ordinate F N is the so-called "mean spherical candle-power of the arc."[1] Similarly, the upper extremities of all lines like P M define a curve X M Y, the ordinates of which define the illumination on a horizontal surface.

The proof is easily given of these propositions. Let the angle D A B be denoted by θ. Let I be the luminous intensity of the arc in the direction of A B. Hence $AB = CL = I$. Let the radius of the semicircle A F be denoted by R. Then if we consider θ to increase by an increment $d\theta$, the corresponding increment of the arc of the semicircle is $R\,d\theta$. If the whole figure revolves round A X as an axis,

[1] See *La Lumière Électrique*, vol. 37, p. 415.

then the area of the zone swept out by this elementary arc is $2\pi R^2 \cos\theta\, d\theta$, and the whole quantity of light falling on the zone is $2\pi R^2 I \cos\theta\, d\theta$.

If we call I_o the "mean spherical luminous intensity," then I_o is defined by the equation—

$$4\pi R^2 I_o = 2\pi R^2 \int_0^\pi I \cos\theta\, d\theta,$$

or—

$$2 R I_o = \int_0^\pi R\, d\theta \,.\, \cos\theta \,.\, I.$$

The right-hand side of the above equation denotes the area of the Rousseau curve G L F, and the left-hand denotes the area of the rectangle N F, F G, where N F is the mean ordinate of the curve G L F. Hence N F represents the mean spherical luminous intensity I_o. Again, the illumination at P on the horizontal plane is equal to $A B \sin\theta / \overline{A P}^2 = B D/(A P)^2$.

Accordingly, if we have the polar curve of luminous intensity of an arc when included in its globe, we can, by means of a simple geometrical construction on the drawing-board, set out a curve whose ordinates represent the illumination produced on a roadway by any number of such arc-lamps placed at any distances and at any heights. For the illumination produced by all the lights is the sum of the separate illuminations. As a general rule, it will not be necessary to consider more than the two adjacent arc-lamps in obtaining the resultant illumination. We are able, therefore, to set out a curve along a line joining the base of the two arc lamp-posts showing the maximum, minimum, and mean illumination in candle-feet, or any other similar units, on the roadway, and decide exactly as to the proper heights and distances of the arcs to obtain a given result.

If the polar curves of luminous intensity of arc-lamps of various descriptions are available, they enable us to make a comparison between them as street illuminants when arranged in any suggested way, without further experiments. Manufacturers of arc-lamps are not, however, in the habit of furnishing this very necessary information, but there is no more reason why they should not do so than that makers of engines should not furnish an indicator diagram for an engine.

With this polar diagram at hand we have the means of predetermining the effect of any proposed arrangement of open or closed, direct-current, or alternating-current arcs, arranged at stated distances or on posts of stated heights. When once the lamps are in position, an illumination survey can be made by means of an illumination photometer, and extensive researches of this character have been carried out in past years by Sir W. H. Preece and Mr. A. P. Trotter.[1] What is required, however, is a means of predetermining the illumination before making expensive experiments. It is usual in these surveys to

[1] "The Distribution and Measurement of Illumination," by Mr. A. P. Trotter, A.M.Inst.C.E. (*Proc. Inst. Civil Eng.*, vol. 110, 1891–1892).

calculate or to measure the illumination either on the pavement surface or at a height of about four or five feet above the pavement. The measurement most required is that of the illumination on a horizontal or vertical surface about five feet above the pavement or road surface, because it is on the illumination at this point that our ability to distinguish objects in the street depends.

Various photometers have been designed for determining by one observation what is commonly called the "mean spherical candle-power" of the arc. M. A. Blondel has devised several appliances for making a measurement of this description. One instrument, called by him the *Lumenmeter*,[1] collects the total flux of the light from the arc, and concentrates it upon a semi-transparent screen, which then forms a secondary focus, the luminous intensity of which is determined. Whilst this appliance is valuable in comparing together the luminous efficiency of different arcs, it does not obviate the necessity for determining the polar curve of luminous intensity, since this last curve, as shown above, affords the means of predetermining the exact distribution of illumination.

III. Heterochromatic Photometry.

We may in the next place consider some interesting questions connected with the photometry of lights of different temperature, and therefore, in general, of different spectral character. Any pair of lights may be defined as *Isochromatic* or *Heterochromatic*, as follows :—

If we form the spectra of two lights and alter the brightness of one of them until the spectra match each other in brightness at one common wave-length, say in the yellow, then if the spectra are matched at all other corresponding points in brightness at the same time, the lights are said to be isochromatic. On the other hand, if the spectra of two lights, when equalised at one common wave-length in brightness, are unequal in brightness at all other points, they are said to be heterochromatic. Thus, for instance, if we form the spectra of a candle and an arc-lamp and reduce them to the same brightness of the yellow, the candle spectrum would be the brighter of the two in the red, but the arc-lamp spectrum would be much the brighter of the two in the violet. Hence, this fact introduces us to the questions : In what sense can we speak of the *candle-power* of an arc-lamp ? Can any scientific meaning at all be attached to this common expression ? The human eye has two distinct powers corresponding to the two principal qualities of a ray of light, namely, a power of colour discrimination, and a power of brightness discrimination. We can pronounce a judgment upon two adjacent illuminated surfaces and say that they are either alike or unlike in colour ; but apart from the colour difference we can also pronounce a judgment in respect of their illumination or brightness. This last judgment is more difficult in proportion as the two patches of light approximate to pure spectral colours. In judging two white surfaces illuminated by pure red and blue light respectively, there is room for a large difference of personal opinion as to their

[1] See *L'Éclairage Électrique*, March, April, May, 1895.

relative brightness, whereas if the two incident lights are very impure, that is to say, much mixed with white light, then it is possible for several persons to make a closely identical judgment as to the equality or inequality in respect of brightness of the illuminated surfaces.

Doubts have sometimes been raised whether we *can* compare differently coloured surfaces or lights in regard to brightness or illumination alone. It is clear, however, that any compound ray, whether reflected from a surface or radiated by an illuminant, can be expanded into a spectrum; and each individual ray compared as regards brightness with the corresponding ray in some standard of light. Hence the effect of the compound ray is to produce a certain resultant or integral brightness which is a consequence of all the separate intensities or brightnesses of the rays of which it is composed, as well as a certain compound colour sensation which is due to the sum of all the separate colour effects of the various wave-lengths.

The opinion has been advanced by Lépinay and Nicati, and has also been supported by Blondel and others, that the eye possesses a *form-* or *detail-* discriminating power, which is not identical with its power of discriminating a difference in brightness. It may be argued, however, that this detail-discriminating power depends essentially upon the brightness-discriminating power of the eye. If, for instance, we are reading a book, or examining a pattern of black lines drawn on a white ground, we have really before our eyes portions of a surface which have unequal reflecting powers, and hence, when illuminated, there is a difference in the brightness of the two parts. We guide the eye along the boundary of a letter by the aid of the difference in brightness between the adjacent parts, and we cannot distinguish any pattern on a surface in which, between the adjacent portions, there is no difference either in brightness or colour. On the other hand, Lépinay and Nicati have asserted that if we equalise, what we may call, the integral brightness on two separate white surfaces, one illuminated by light of one kind, say blue, and the other illuminated by light of another kind, say red, then we can more easily distinguish a black pattern drawn upon the surface illuminated by red light, than upon the surface illuminated by the blue. The above investigators have stated that if yellow and blue light produced by any prismatic means are adjusted to produce equal apparent brightness when falling upon two parts of a uniform white surface, then when these rays are allowed to fall upon a printed book the type is more easily read which is illuminated by the yellow light than that illuminated by the blue.[1] It may therefore be the case that the integral brightness of lights of different spectral compositions is not a measure of their power of bringing out a detail printed in black on a white ground. If so, we must include amongst the powers of the eye a definite detail-discriminating power, and among the qualities of a ray of light a detail-revealing power, understanding by this term "detail" a fine pattern of black on white, such as a printed page or handwriting, or fine

[1] *Macé de Lépinay* and W. Nicati, *Journal de Physique*, vol. 2, p. 75, 1883.

black lines or black dots on a white ground. It may be noted in passing that we cannot detect the existence of a black line on a white ground unless the width of the line subtends a certain angle at the eye. A line having an angular width of 1′ can certainly be seen. This corresponds with a line 1 mm. in width viewed at a distance of 344 centimetres, or about 11 feet, but a black line on a white ground having a width subtending an angle 1″ certainly cannot be seen.

This ability to see a black line on a white ground is dependent on the illumination of the surface. Thus, if we draw a series of parallel black lines of gradually increasing width, we shall find that they cease to be visible one by one, either as we move farther away from them, or as the illumination on the paper is decreased. The same thing is true of white spots of various angular magnitudes placed on a black background. Hence, the power to discriminate a number of black lines having a given angular width ruled on a white surface, may become a measure of the illumination of the surface. If on a white ground we place a number of black lines or black dots, or on a black ground a number of white dots of such a diameter that at the distance of distinct vision they subtend an angle 1′; and if we illuminate one portion of this ruled or dotted surface, one by a standard illumination and the other by a light under test, we may assert the two portions to be equally illuminated when we can with equal ease or sharpness distinguish the pattern on the two portions.

This statement, however, is largely qualified by the reflex power of the pupil of the eye to vary in aperture according to the illumination of the object regarded. If we could control the aperture of the pupil, there might possibly be a definite discriminating power corresponding to each grade of illumination. As it is, the lower the illumination the more we "strain" the eye in the effort to see detail.

It appears that this method of judging the equality of illumination of two parts of a white surface does not lead to quite the same results as the process of judging the integral brightness of the surfaces. Lépinay and Nicati have accordingly distinguished these two methods by calling them the "method of equal brightness" and the "method of equal distinctness or sharpness," and M. A. Blondel, in discussing this subject, has pointed out that we may distinguish the value of a standard according to its "luminous intensity" or its "visual intensity," or we may perhaps translate these expressions somewhat freely by calling them "*the power of creating brightness*" and "*the power of revealing detail.*" [1]

The purposes for which we require artificial light are partly for revealing what we call the colour differences between objects, and partly for revealing detail. In the one case the chromatic quality of the light is of great importance. In the second place, the chromatic quality is not of so much importance, provided it is accompanied by

[1] The reader may be referred to a paper by M. A. Blondel read before the Congress of Electricians at Chicago, 1893. See also *The Electrician*, vol. 32, p. 117, for curves representing the distribution of *visual intensity* and *luminous intensity* in the spectrum; also for an excellent discussion of the problem of heterochromatic photometry.

sufficient intensity or brightness. Thus, for the purpose of reading, we are far less concerned with the chromatic quality of an illumination than we are when we are providing an illuminant for a picture gallery or a dye-house.

Another matter in connection with heterochromatic photometry of special interest is that known as Purkinje's phenomenon, which is intimately related to the general law connecting stimulus and sensation. The above phenomenon is best illustrated by the following experiment :—

We take a white right-angled wedge and illuminate one side by a red light, and the other side by a blue light, and adjust the distances of these two illuminants until we obtain what we consider is an equal illumination on the two adjacent sides of the wedge. If we then move in both these lights to half their distance from the wedge—in other words, make the *objective brightness* of the surfaces fourfold, we find that the retinal stimulation or the *apparent brightness* of the two surfaces are no longer the same. It is therefore clear that although the retinal sensation of brightness increases with the objective or actual illumination of the surface, it does not increase according to the same law for all colours.

Fechner's law connecting sensations and stimulus is sometimes stated in the form that *sensation varies as the logarithm of the ratio of stimulus to minimum stimulus*, understanding by the latter term the stimulus which has to be applied before any sensation at all results.[1] Hence, it is clear that if we could set out in the form of a curve for the different colours, the objective intensity and the retinal sensation for different colours, these would be represented by different curves. The curve corresponding to a red light would be steeper than the curve corresponding to a blue light (see Fig. 9). It follows from this that if we illuminate such a simple wedge photometer on one side by a candle, and on the other side by an arc-lamp, the ratio of the distances at which these two illuminants must be placed in order to produce what we consider to be an equal brightness on the two surfaces, will depend upon the degree of that surface brightness. Hence, there is no fixed ratio between the luminous intensity or illuminative power of an arc-lamp and a candle in regard to the brightness they produce on a white surface, apart altogether from their colour difference.

In view of these facts, great authorities, such as Von Helmholtz, have declared that there was no such thing as heterochromatic photometry; in other words, no possibility of defining in any scientific sense the candle-power of an arc. With regard to colour-distinguishing power, or colour-revealing power, it is perfectly clear that no scientific meaning can be attached to the term "candle-power of an arc." Our standard light, as regards revealing the so-called natural colours of

[1] If S stands for sensation and I for stimulus, and i for the least stimulus which will create recognisable sensation, then according to Weber's investigations on Fechner's Law $S = k \log \frac{I}{i}$, where k is some *constant of sensation*. See also Principal C. Lloyd Morgan, F.R.S., on "Studies in Visual Sensation," *Proc. Royal Soc.*, vol. 68, p. 459. The Croonian Lecture, *March 21, 1901.*

objects, is *daylight*, say the light from a northern sky, such as that which an artist admits to his studio. The same surfaces viewed by the aid of other illuminants may create totally different sensations in the eye, and it is a question whether any single numerical coefficient can be attached to these illuminants defining their colour-revealing powers in terms of daylight, taken as a standard. On the other hand, if we separate out the sensation of brightness from that of colour, we can then define the power of an arc in terms of that of a candle, as regards its power of producing brightness on a white surface, *provided we define what that brightness shall be*. If we take as our standard of brightness one *candle-foot*, that is, the illumination produced by one candle placed at a distance of one foot from a white surface, then we can by one single number express the ratio of the two lights in producing brightness of this kind, but the same ratio is not applicable to other degrees of brightness, and hence, generally speaking, there is no such thing as an absolute "candle-power of an arc." In consequence of Purkinje's

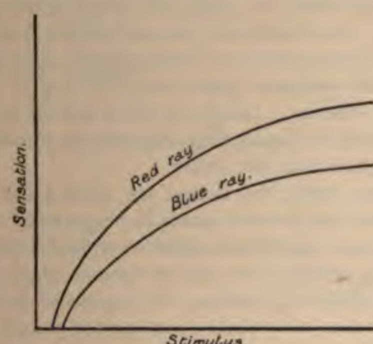


FIG. 9.—CURVES ILLUSTRATING THE PURKINJE PHENOMENON.

phenomenon, an arc-lamp has less candle-power the nearer the eye is to it.

It has therefore been proposed that we shall define the ratio of two heterochromatic lights by means of their detail-distinguishing powers. This could be done if we agree on a certain pattern which shall afford, as it were, a standard of discrimination. Suppose, for instance, that we rule on a white surface a number of black lines 1 mm. wide and 1 mm. apart, then the width of these lines subtends an angle of 1′ at a distance of 3·44 meters. We can by photography reduce this diagram to such dimensions that the lines subtend the same angle at a distance of 10 inches, which is about the distance of distinct vision. If we cover the two sides of the Ritchie wedge or a Bunsen disc with two pieces of paper equally ruled with the above pattern of lines, or on which the above standard has been photographed, then we might define the equality of brightness or illumination on the two surfaces of the disc or wedge as being those which enable us to distinguish with equal ease the ruled pattern on the two surfaces. The same test can

be applied using white dots of given angular magnitude on a black ground as the test object. Such an equalisation may be called an equalisation of distinctness, as compared with an equalisation of mere surface brightness on two white surfaces. The first may be called the *method of equal distinctness*, and the second may be called the *method of equal brightness*. The two methods do not lead to precisely the same results when we employ two such illuminants as a candle and an arc-lamp.

Some of the above statements have been tested by experiments recently made in the Pender Laboratory at University College, London. The Purkinje phenomenon can be easily shown in the following manner: A cube of wood is covered on two adjacent sides with fine white card. Two incandescent lamps are placed in wooden boxes, the front of one being covered with red glass and the other with bluish-green. The kind of glass that is convenient to use is that employed for railway signals, known as ruby red and signal green. These lamps are placed on either side of the cube, so that one surface of the cube is illuminated by red light and the other by green, the sides forming equal angles with the rays of light. The lamps are placed about 6 feet apart. The cube must then be so placed that on looking at the edge from a distance of 4 or 5 feet the two surfaces appear equally bright, one being red and the other green. The two lamps are then moved in each to half their distance, and it will be found that the side illuminated with red light is now much brighter than the other.

If, on the other hand, the lamps are moved away to double their distance, the green side predominates in brightness.

Experiments have also been made to obtain a standard for testing the discriminating power of the eye in various illuminations. This has been done in the following manner: It is possible to buy a certain kind of black printed calico with white spots upon it placed at equal intervals, these white spots being 2 mm. in diameter. If a square of this material is photographed, it is possible to obtain a photograph consisting of black spots on a white ground of such a size that when the photograph is viewed at a distance of 10″, which is the distance of distinct vision, the diameter of the spots subtends an angle of 1′. By preparing two paper photographs of this description, consisting of black or white dots having an angular magnitude of 1′, and placing these photographs on either side of a Ritchie wedge, it is easy to balance two lights placed on the two sides of the wedge, not with regard to their power of making equal surface brightness, but with regard to their power of equally revealing detail.

A difficulty, however, that has presented itself is that this photometric method does not appear to be sufficiently sharp. One can change the intensity of one of the lights by a percentage which would make itself at once evident in an ordinary photometer, without changing in a very marked degree the discrimination of the detail. This, however, is undoubtedly connected with the reflex power of the eye to accommodate itself to different illuminations on the surface regarded. If the illumination falls off on one side, the pupil of the eye, in looking at that surface, immediately expands in the effort to search for more

detail. This matter does not seem to have received sufficient attention in many researches in which the power of the eye to see fine lines has been used as a measure of brightness.

The author has, however, found that the difficulty arising from the reflex adjustments of the pupil of the eye can be greatly, if not entirely, overcome by the following simple device :—A thin metal plate is placed in front of one eye, which is pierced with an aperture 1 mm. in diameter, and through this small opening the diagram on the photometer disc or wedge is examined. The light that reaches the eye is therefore limited by the angular aperture of this small opening, and no alteration in the pupil of the eye, within the limits naturally occurring, can influence the amount of light entering the eye. Nothing can alter it but the illumination of the disc looked at.

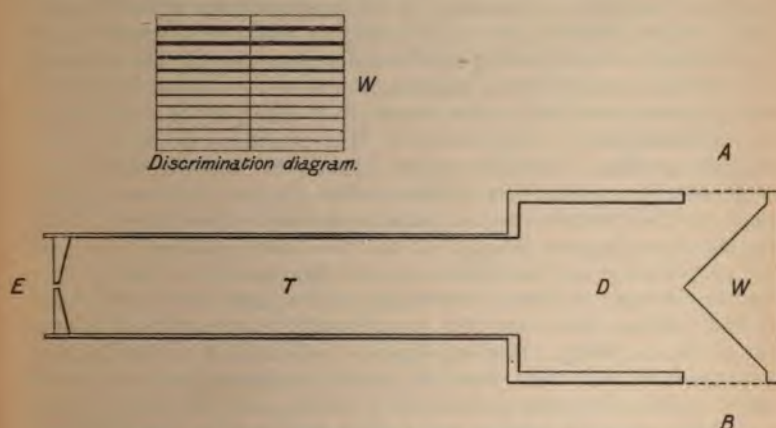


FIG. 10.—DISCRIMINATION PHOTOMETER (FLEMING).

W. Ritchie Wedge covered with Ruled Discrimination Diagram W.
E. Pinhole Eyepiece. A and B. Two Sources of Light. T. Eye-piece Tube.

During this examination of the disc through the small aperture the other eye must, of course, be closed. Any one can try the experiment by pricking a hole in a visiting-card with a large pin, and holding this aperture exactly in front of one eye.

If, aided by this simple device, we examine a sheet of black paper, or black calico, on which there are small white spots or narrow white lines, it will be found that as the illumination is gradually decreased upon the screen, the spots vanish with extreme sharpness at one point. It follows, therefore, that by the use of such an "artificial pupil" of constant size, the process of photometry by the discrimination of a standard black and white pattern is rendered much more exact.

The arrangements most convenient to use for such a *discrimination photometer* are a brass tube, having at the end close to the eye a brass plate pierced with an aperture 1 mm. in diameter (see Fig. 10). By the aid of this tube we examine a *Ritchie* wedge, the two sides of which are covered

over with white paper ruled with black lines of various widths and placed on the sides of the wedge, or else with dead black paper, on which there are small white spots or parallel white threads one-fifteenth of a millimetre in diameter. These spots or lines, when regarded from the distance of most distinct vision—namely 10 inches—have an angular magnitude of 1 minute. Such a standard paper can be prepared by photography without difficulty, and it may be possible to agree upon a *standard discrimination pattern*, consisting of such white dots one-fifteenth mm. in diameter, placed, say, 1 mm. apart, and examined at a distance of 250 mm. through an aperture 1 mm. in diameter placed close to the eye. Two lights are then said to produce equal illuminations on this disc, if when so regarded we see the pattern on each part with equal distinctness.

The question which remains to be decided is which of these two methods best gives us a measure of the practical value of the illuminant. Before, however, we can construct a photometric method, we must know the law according to which the effect varies. The assumption which lies at the root of all present methods of photometry is that the brightness of a surface varies inversely as the square of the distance of the illuminating source from it. The postulates made are that equality in retinal sensations implies equality in objective brightness or illumination, and that actual illumination must vary inversely as the square of the distance from the luminous source, in consequence of the rectilineal propagation of light. If the detail-distinguishing power of the eye does not vary *pari passu* with the illumination of the surface, we have no right to assume that we can read equally well by the light of four candles placed two feet away from the page as by one candle placed one foot away. If an arc-lamp at a distance of 10 feet renders a certain black and white detail as clearly as a candle placed one foot away, we can infer nothing about the "candle-power" of the arc until we know how many candles placed at 10 feet away are equivalent in detail-revealing power to one candle placed one foot away from the surface. Experiment shows that the detail-revealing power of four candles at two feet, and nine candles at three feet, and one candle at one foot are practically the same. Hence, if in the above case of the arc and candle we find that an equality of detail-revealing power results from the arc at 10 feet and the candle at one foot, we can say that the arc in question has 100 candle-power. Suppose, however, we compare the integral brightness of two adjacent white surfaces produced by these two illuminants respectively, at the distances named, we should then probably find that the arc-illuminated surface looked brighter than the candle-illuminated surface. The arc-light contains rays that practically add nothing to its detail-revealing power, but they do add to its integral brightness-producing power.

The sum and substance of the foregoing discussion is that our methods of defining luminous intensity are still imperfect. The term "candle-power of an arc" has no scientific precision as at present used, and we must seek for some better basis for the numerical evaluation of an arc-lamp.

The difficulties of heterochromatic photometry, and especially the

personal difficulty experienced by many observers in distinguishing between the general brightness of two surfaces and their colour difference, has led to many suggestions with the object of putting the photometry of lights of a different spectral character on a more certain basis. Two of these methods are sufficiently important to deserve mention here. The first is that due to Crova, which is based upon absorption. The old plan of holding a piece of red or green glass in front of the eye when comparing, by means of the photometer, an arc-lamp with a candle or glow-lamp is, of course, utterly unscientific, and the figures obtained of no value whatever. Crova's method depends on the fact discovered by him (see *Comptes Rendus*, vol. 65. p. 572) that the integral brightness of two nearly white lights are in the ratio of the brightness of the rays in them having a wave-length $\cdot 582\mu$. Hence, if by means of an absorbing medium we select from two heterochromatic lights the rays approximately of this wave-length and determine their relative intensity, we have, according to Crova, a figure which gives us the relative brightness of the two lights. For this purpose, Crova employs a solution consisting of sublimated anhydrous ferric chloride 22·321 grams, crystallised nickelous chloride 27·191 grams, dissolved in distilled water, and the volume brought up to 100 cubic centimetres at 15 degrees centigrade. This solution is placed in a glass trough, and transmits radiation of a wave-length lying between $\cdot 630\mu$ and $\cdot 534\mu$ having a well-marked maximum at $\cdot 582\mu$.[1] If this trough is held in front of the eye when making a photometric comparison between an arc lamp and a candle, and if the distances of the illuminants are adjusted so that the two parts of the photometer disc seen through this solution are of equal brightness, then, according to Crova, the luminous intensities of the two lights are inversely as the squares of their distances from the screen. It is certain, however, that Crova's method can only be applied within limits. If the spectral character of the two lights is very different, it is unquestionably inapplicable.

Another method for eliminating the colour difficulty in the photometry of dissimilar lights is by employing the method of "flicker" suggested by Professor O. N. Rood in 1893.[2] Rood constructed a photometer as follows:—A Ritchie wedge with white surface has placed in front of it a prism or a lens in such a manner that on looking through it the eye sees only one of the surfaces of the wedge at a time. If the prism or lens is made to vibrate rapidly, so that alternate glimpses are obtained of the two sides of the wedge, we have what is called a "flicker photometer." If the two sides are illuminated by heterochromatic lights, then on employing the vibrating prism the difficulty of determining when the two sides on the wedge are equally bright, apart from their colour difference, is reduced.

The principle of the flicker photometer was discovered by Professor Rood in the course of some experiments with a Maxwell colour top.

[1] μ = ·001 millimetre. *Comptes Rendus*, vol. 119, No. 16, p. 627; Oct. 15th of *Electrician*, vol. 33, p. 754; or Palaz, *Traité de Photometrie Industrielle*, p. 82.

[2] *Science*, vol. 7, 1898, p. 757. Also *Science Abstracts*, vol. 2, Abstract 26. Also see *American Journal of Science*, vol. 46, p. 173, 1893.

The principle involved is that if two surfaces are alternately presented to the eye, which are differently illuminated, then a certain peculiar flicker is produced, which is destroyed if the surfaces are made to be equally bright. The two surfaces must, however, alternately fill up the whole field of view of the eye, and be, as it were, superimposed. The flickering effect then disappears, provided that the surfaces are equally bright, no matter whether they are of the same colour or not.[1]

Another variety of flicker photometer has been described by Professor F. P. Whitman.[2] From a circle of white card, which is fixed to the axis of a motor, he cuts away a semicircular segment. This disc is placed on the photometer bench so as to make an angle of 30 degrees with the line of light. Behind this disc is placed another fixed sheet of white card, inclined at an angle of 60 degrees to the revolving disc. The two lights to be compared are placed on either side of this arrangement, and on revolving the disc and looking in the proper position we obtain intermittent glimpses of the white card behind. The brightness of the two surfaces—namely, that of a disc and the card—are then equalised by moving the lights, and this equality in brightness is known to exist when the "flicker" just vanishes. The author has made a slight modification of the above form of flicker photometer by employing a white card disc cut in the form of a Maltese cross, with the open sectors equal in magnitude to the cross arms, and using the disc on the axis of a motor as above described (see Fig 11). Another compact form of flicker photometer devised by the writer contains in a box the screen and fan driven by clockwork at just the right speed. It is quite an easy matter to compare by means of it an arc-lamp and a candle.

Mr. A. Vernon Harcourt has mentioned in a paper on "Photometry by the Pentane Standard" that his attention was drawn by Mr. Dibdin, chemist to the late Metropolitan Board of Works, to the fact that a *star disc* affords a much better means of comparing heterochromatic lights than the simple Bunsen *grease-spot disc*. In using the ordinary Bunsen grease-spot disc for the comparison of heterochromatic lights, the writer has found that the colour difficulty is partly eliminated by throwing the eye out of focus. In other words, not endeavouring to obtain too sharp a view of the object looked at.

It was shown in 1877 by Charpentier, and Donders in 1880, that the so-called *yellow spot* in the retina of the eye is less sensitive than the remainder of the retina to the most refrangible rays. Hence the judgment which is formed as to the relative brightness of two adjacent surfaces different in colour, will depend upon the angular magnitude of these surfaces. If the two surfaces do not subtend an angle at the

[1] According to the investigations of E. S. Ferry on "Persistence of Vision" (see *American Journal of Science*, vol. 44, September, 1892), the duration of a visual impression upon the eye is not much affected by colour, but almost entirely determined by brightness, and the duration (D) varies inversely as the logarithm of the brightness (B) of the light, or

$$D = \frac{1}{k \log B}.$$

[2] *Science Abstracts*, vol. 2, Abstract No. 28. Also *Physical Review*, vol. 3, *p. 241, 1896*. See *Proc. Brit. Association*, Aberdeen, 1885.

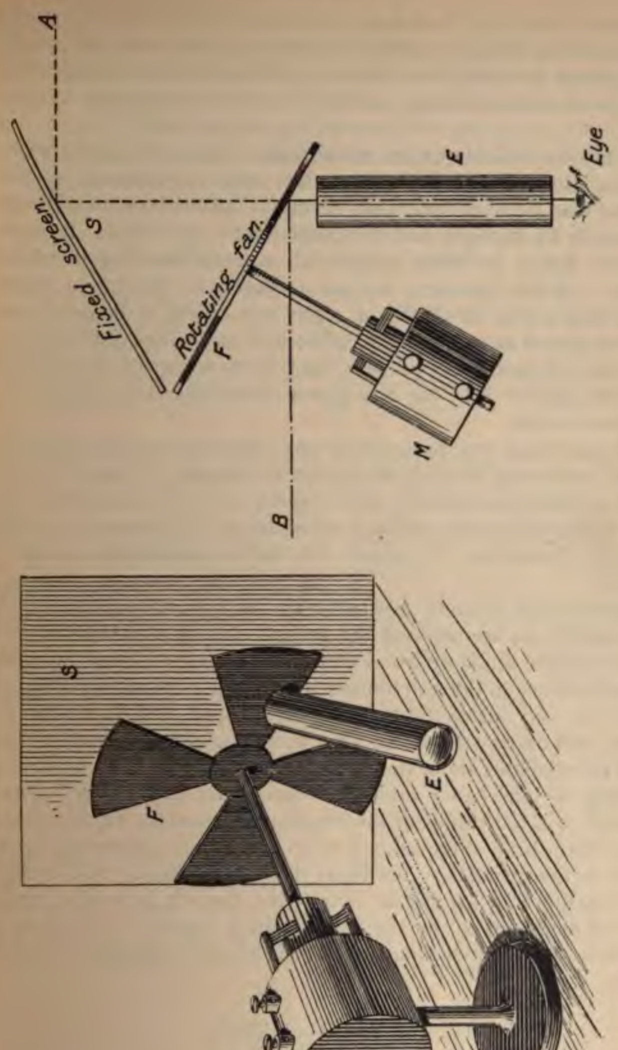


FIG. 11.—FLICKER PHOTOMETER.

M. Motor carrying White Maltese Cross Fan F. S. Fixed Screen. E. Eye-tube. A and B. Two Sources of Light.

eye greater than 45′, then there is a selective action in the eye which reduces the colour difference between the two patches, and renders it more easy to decide as to their relative brightness. If, however, the patches to be compared are much smaller than this, there is a considerable increase in difficulty in deciding as to their general brightness.

Before *heterochromatic* photometry is placed on a perfectly satis-

factory basis, we require to determine and to define more closely what it is we are comparing, or desire to compare. We want, if possible, some means of defining how far any given illuminant differs from daylight in colour-revealing power for certain standard coloured surfaces.

This may be difficult to do numerically, but it is probably not impossible. Considering various surface colours produced by paint or dyes, we find, for example, that certain shades of green affect the eye differently by daylight and by candlelight or arc-light. It may be possible to define in some numerical manner the degree of this difference. If, for instance, we are engaged in lighting a picture gallery so that it can be visited at night, our object is to produce an illumination which as nearly as possible will reproduce daylight in its effect on the pictures. At present there is no means of describing in any definite manner how far any given illuminant is appropriate for this purpose or not.

In the next place we require to arrange and define a standard detail pattern, in order that we may determine the quality of any given light in revealing detail; and finally, with respect to the integral brightness, we have to determine and define a standard of brightness or illumination in the production of which the lights considered should be compared.

The mere term "candle-power of an arc" is unquestionably too vague to satisfy requirements at the present day, in defining the value of a given electric arc-lamp as an illuminating agent. Apart altogether from the difference in the distribution of the light created by continuous-current arcs, and alternating-current arcs, open and enclosed arcs, flame arcs, and other varieties of arcs with specially prepared carbons, there are peculiarities about the light emitted from these various forms of arc-lamp which render them best suited for particular forms of illumination, and not sufficient information of scientific value is gained by simply describing them as arcs of so many mean spherical candle-power, or so many watts. Our ideal light is daylight. We ought to be able to define how far any artificial light can act as a substitute for daylight in enabling us to see surrounding objects in their proper colours, with their proper details and their proper brilliancy or relative brightness.

IV. Photometric Units.

We may in conclusion make a brief reference to the subject of photometric units and International agreements thereon. The Congress of Electricians at Geneva in 1896 adopted a nomenclature as follows :— They accepted as the names of the five fundamental photometrical quantities the terms, (1) *Luminous intensity*, or *Intensity*. (2) *Luminous flux*. (3) *Illumination*. (4) *Brightness*, or *Intrinsic brightness*. (5) *Lighting*, or *Quantity of Light*.

They adopted as the unit of *luminous intensity* the *candle*, and defined it as being practically represented by the *decimal candle* or *bougie-decimale*, which a previous Congress in 1889 at Paris had defined as the twentieth part of the light emitted normally by one square

centimetre of platinum at its melting point. The 1896 Congress further asserted that this decimal candle might be practically represented by the Hefner unit.[1] As the unit of *luminous flux* they adopted the word *Lumen* to signify the light sent out from a unit source through a unit solid angle. Following the decision of the Congress held in 1893 at Chicago, they defined the unit of illumination as a flux of one lumen per square meter, and it *a Lux*,[2] and the other units were specified as in the table below :—

Photometric Quantities.	Units.	Symbols.
Luminous Intensity ...	The Candle	I
Luminous Flux	The Lumen	Φ
Illumination	The Lux	E
Intrinsic Brightness ...	Candles per square centimetre	i
Quantity of Light ...	The Lumen-hour	Q

The resolutions of Congresses are often carried at the instigation of one or more influential or persuasive speakers, but it is a matter of regret that we have not in these matters what the politicians call a *Referendum* to the general body of electricians. No Congress can force a term into use which does not commend itself to the mind of the ordinary worker. In this case one cannot but wish that the resolutions adopted had been previously more discussed in the technical press.

In the first place, the *Candle* is too small a unit of luminous intensity for the purpose of electric photometry. The unit of lighting adopted now for a long time past by electrical engineers has been the 30-watt glow-lamp which when working at 3 watts per candle gives a light of 10 candles. Furthermore, electricians are accustomed to reckon out the whole of the lighting of a supply station which is conducted partly by arc-lighting and partly by glow-lamps of various sizes, in its equivalent in 30-watt glow-lamps, which used to be called 8-candle lamps, but as a matter of fact are nearer 10-c.p. Again, a 10-candle lamp is now the photometric unit adopted in gas-light photometry by the Gas Referees. The Carcel lamp, the French official standard, has a value not far from 10 candles. Hence the candle is becoming a thing of the past, both as a practical illuminant and as an actual standard in photometry. We have got rid of the article itself, why should we retain the name? It is like continuing to reckon lengths in "barleycorns," three of which were said to make an inch; and if the candle is no longer in use in practical photometry, it will soon have to be expunged from the Statute book as the legal unit of light. At the present time it may almost be called archaic, a thing to be preserved in museums, but not to have its name perpetuated as a unit of light in every way too small for modern purposes. A unit of light of

[1] See *Rapport sur les Unités Photométriques*, par M. A. Blondel. *Congrès International des Électriciens*, Genève, 1896.

[2] The word *Lux* was originally suggested by Sir W. H. Preece, at the 1889 Paris Congress of Electricians, as the name for a unit of Illumination, and applied by him to express an illumination equal to a *Carcel-Metre*, nearly equal to one *candle-foot* in magnitude.

convenient magnitude for the purposes of electric lighting is that which is given by the 30-watt glow-lamp or by the Harcourt pentane lamp as adopted by the Gas Referees.

Instead of calling this standard of luminous intensity *ten candles*, why not call it *one lamp?* The word *lamp* is a short, common word existing both in French and German, and therefore not presenting anything strange in sound.[1] A light which we now call 10-*candle power* would be called *one-lamp power*, and similarly lamps of 20-, 50-, and 100-candle-power would then be called lights of *two-lamp power*, *five-lamp power*, and *ten-lamp power*. These simple multiples are more convenient than the present 8-, 16-, and 32-candle multiples which are in use for glow-lamp classification. These last multiples were only adopted originally because at the outset electric-lighting people copied gas-lighting people in everything. We put our wires originally into gas brackets, fixed our electric lamps to gas chandeliers, and selected as the standard glow-lamp one which gave the same light as an argand gas-burner consuming 5 cubic feet per hour. It would appear, therefore, that a case can certainly be made out for reckoning luminous intensity in larger units than a candle, each of which is called *one-lamp power*, and equivalent to what we now call 10-candle power.[2] This would have another advantage, because the unit of brightness or illumination would then be the *Lamp-metre*, namely, brightness produced by a luminous intensity of one lamp on a white surface at a distance of one metre. This brightness would be very nearly equal to a Carcel-metre, originally named a Lux by Sir W. H. Preece, and to that which we call *one candle-foot*, a convenient illumination for the purposes of vision. The candle-metre or Bougie-metre christened by the 1896 Congress one Lux is too small an illumination to take as a standard. An illumination of one lux on a printed page is not sufficient to enable us to read. It is about the illumination given on a newspaper in the hands of an unfortunate traveller in a railway carriage illuminated by one of the miserable oil lamps still in use on some lines. The least comfortable illumination for discriminating print is one 10 times as great as that called by the 1896 Congress *a lux*. The practical photometerist hardly ever feels the need for other units than those of luminous intensity and illumination. In the suggested nomenclature the unit called the *lamp* would be the name for the first, and the *lamp-metre*, which might also be called lux if desired, would be the unit for the second.

If the Violle platinum standard should be ultimately adopted as the final standard of reference in Great Britain, the *lamp* might be defined as that light given out normally from half a square centimetre of the surface of platinum at its melting point. It could be reproduced or recovered at distant places by the use of a Harcourt pentane argand lamp, or by the use of one of the large bulb electric glow-lamps taking 30 watts.

[1] French, *La Lampe*; German, *Die Lampe*.

[2] Although so-called 5-candle lamps are much used in electric lighting, no difficulty would arise in speaking of these as *half-lamp power*, and the $2\frac{1}{2}$-c.p. lamps as *quarter-lamp* power.

Again, why do we still adhere to that rather absurd method of measuring what is called the "efficiency" of an electric lamp by stating the *watts per candle?* This so-called efficiency is greater the less the number by which it is defined. We ought rather to specify this quantity in *lumens per watt or per kilowatt.* In lumens per watt the efficiency is a number near to 4 for a glow-lamp and 12 for a continuous-current arc, and these numbers give us at once some idea of the relative economy in working. We prefer, however, apparently to travel along old intellectual grooves rather than strike out for a new and better way.

A more important matter, however, than the reorganisation of nomenclature is the actual establishment in England of a primary reference standard of light. At present there is no Court of Appeal in case of disputes as to the so-called candle-power of glow-lamps or arc-lamps. Without presuming to dictate a course of action, it is much to be desired that this matter should engage the attention of the National Physical Laboratory without delay, and that a careful re-investigation should be made of the Violle platinum standard, and at the same time the Lummer and Kurlbaum platinum standard as adopted by the Reichsanstalt should be examined. Also experiments should be undertaken to see how far the large bulb glow-lamps made on the plans suggested by the author can be employed as a means of distributing or reproducing this standard in distant places.

The Hefner lamp has rightly never been accepted in Great Britain, as it is not a suitable practical unit for electric photometry, but if the platinum reference standard could be set up at one or two places, and if it could be shown that the light from one square centimetre of molten platinum is practically represented by twice that given by a Harcourt pentane lamp, or by a certain glow-lamp made and used in a certain manner, the difficulties which beset photometry at the present moment from the want of a common recognised standard would be diminished. It is unfortunate that national feeling seems to enter into this question of the selection of standards of light. No sooner is one practical unit suggested in France than a different one is adopted in Germany and a third in England; but that is no reason why the whole question should not even now be re-examined *ab initio* by photometrical experts with the object of settling an International Unit of Light, and obtaining for it universal acceptance as in the case of the International Electrical Units.

These suggestions are thrown out not in any dogmatic spirit, but as the result of nearly twenty years' experience in the testing of electric lamps, and in the hope that they may stimulate discussion and enable the members of this Institution to formulate their experience and opinions on the photometry of electric lamps.

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Mr. A. Vernon Harcourt: It is very difficult to be called upon to make remarks upon this paper, because of the great amount of matter that has been brought before us, and the great variety of materials. Perhaps I may take what is the main subject of the paper, namely, the photometry of the electric arc. Many years ago I took part in the photometry of arc lights in connection with an investigation made by the Elder Brethren of the Trinity House into the value of various sources of light placed behind lighthouse lenses. There were set up at the South Foreland some temporary lighthouses, in one of which the source of light was an oil-lamp, in another a gas-lamp, and in another an electric arc; and these were piled one upon another, so that there were three or four tiers, in order to produce the most powerful light possible. The object was to test the value of these different lights in different kinds of weather. Thus the inquiry extended beyond the matters which have been dealt with to-night, but it involved the photometry of the various lights, by themselves or as seen through lenses, on clear nights as well as on hazy nights. There was not found to be any insuperable or even great difficulty in arriving at a comparison between Mr. Vernon Harcourt.

Mr. Vernon Harcourt. the arc lights and the large flame lights. The measurements were chiefly made at a considerable distance. The Trinity House provided some huts for making measurements at distances of one, two, and three miles from the lighthouses, and the beam of light sent from the several holophotes could be thrown upon them.

The point, however, to which I wish to refer is that it was found possible to arrive at a fairly accurate estimate of the value of the arc light by itself, and as shown through lenses, by means of a Bunsen photometer, in which in place of a plain opaque disk with translucent margin a translucent disk with serrated rim and opaque margin was used. This small difference greatly facilitated the estimation of the illuminations produced by the distant arc light on one side and the one-candle pentane flame a few feet off on the other. The appearance of the two illuminations was, of course, exceedingly different, but I found it was possible to make the comparison by fixing attention upon the degree of distinctness of the central figure, the star, against the background, and that it was not difficult to do this without being distracted by the difference of colour. On one side was seen a blue star upon a pink ground, on the other side a pink star upon a blue ground. I had a good opportunity of trying the general practicability of this comparison by asking a number of the mechanics and other servants of the Trinity House—men not practised in photometry, but intelligent men with good eyesight—to place the photometric disk in succession. The direction given them was to bring the disk to such a position that the stars on the two sides as seen against the background, should have an equal, minimum, distinctness. If the disk were moved thence a little to the right or to the left the star of one or the other colour stood out more clearly, and it was not difficult by sliding the disk-holder to and fro to arrive at a position in which the distinctness of the pattern was equal. The photometer was so arranged that the observer for the time being could not himself see what the reading was, but another man read the scale on the opposite side of the photometer, when directed to do so, and wrote the figures down without saying a word. In this way of working there was no possibility of self-delusion and of an artificial consistency, such as happens especially with a photometer where the disk is moved by a winch. It was found that the results of observations so made showed a very fair amount of agreement; I do not remember what the percentage error was, but it was not large.

The mention of moving the disk backwards and forwards reminds me of what I was shown some years ago by Sir William Abney, who pointed out that a good judgment may be made between lights of different colours by using a comparatively large oscillation. Certainly we have, apart from our perception of different colours, an impression of brightness; we can be quite sure that a very bright green light is brighter than a dull red light, or *vice versâ*. Beginning with rather large oscillations of the disk-holder and gradually diminishing their amplitude we can arrive at a middle point representing equal illuminations. The colour difficulty may also be overcome by using *very low illuminations*, such as that of one candle at a distance of eight *or ten feet.* In the dusk the leaves and petals of a scarlet geranium

can scarcely be distinguished by colour, though their forms are still quite visible. So it was with the illumination of adjoining strips of the photoped by a one-candle flame ten feet away, and the beam from the lighthouse arc at a distance of two miles. Mr. Vernon Harcourt.

I should like to say, in conclusion, that this paper appears to me to be one of very great interest. I have listened to many papers on different points in photometry, but never to so complete an account of the subject as we have had the pleasure of hearing this evening.

Dr. R. T. GLAZEBROOK: I should like to re-echo what has just fallen from Mr. Vernon Harcourt as to the extreme interest and value of the paper to which we have listened. I feel that Professor Fleming should be thanked very cordially for having brought this matter before us in so clear and interesting a manner, and I think the Institution is to be congratulated in having such a paper laid before it. I have a particular reason for being grateful to the author for the paper, because he has called attention to the important work on the subject that still remains to be done, and has made suggestions of great interest and value on various points as to how that work may be taken up by the Institution which I direct. I say I have reason to be thankful because, although it is the fact that so many suggestions in regard to work which may be undertaken by the National Physical Laboratory have been made that I fear it will be long before we can carry them all out, yet I am glad to have suggestions made, in order that I may go to the powers that be with a still stronger case to prove to them the utility and necessity of that Institution. However, with regard to photometry we are perhaps in a better position than we are with regard to some other questions, because, although we have done nothing as yet, we are, thanks to the generosity of Sir William Preece, Mr. Trotter, and some others, before long to be equipped with a very complete set of photometric instruments and appliances, and then I hope we shall go into the question thoroughly and completely, and standardise lamps for the users as is done in Berlin. Dr. Glazebrook.

There is very little to criticise in the paper. With most of it I agree very cordially indeed. It struck me on looking it through that possibly the position that Professor Fleming gives to the Vernon Harcourt 1-candle pentane standard in comparison with the Hefner or amyl acetate lamp was not one that would be assigned to it by all writers on photometry. If one takes the figures which Professor Fleming gives in the paper, it appears, for example, that the effect of change of barometric pressure on the Vernon Harcourt 1-candle flame standard is four or five times as great as the effect of similar changes upon the amyl acetate lamp. That in itself seems to me to be a difficulty if one wished to criticise or discuss exact details. I should say further with regard to the 10-candle pentane lamp that I am not quite sure from the paper if the question of the effect of change of atmospheric pressure, moisture, the presence of carbonic acid, and so on, has as yet been examined in the same detailed way as it has for the amyl acetate and for the 1-candle pentane lamp. I think I am right in saying that that examination has not been made so completely, and that it is one of the pieces of work we may well take up in time.

Dr. Glazebrook.

I was more especially interested in what Dr. Fleming told us about the possibility of arriving at an incandescent standard of light, and in particular to the reference he made to the recent and very valuable and important work of Mr. Petavel. I have read that work with very great care, and I value it very highly; indeed, I hope in a few moments to bring before the Institution some of the more recent developments of Petavel's work, which I think have added greatly to its value. At the same time I am inclined to think that Mr. Petavel is a little too sanguine in the expression of his opinion as to the possibility of realising the Violle standard. He succeeded in doing that, though with great difficulty, by means of the precautions that are described in his paper. I should have been more hopeful had he succeeded in melting his platinum by means of an electric current. But he found difficulties in using the electric current to melt the platinum, and had, in consequence, to have recourse to a very careful mixture of oxygen and hydrogen, using the oxy-hydrogen blowpipe. There are, however, one or two methods in which we may hope to use the surface of incandescent platinum, made to incandesce by means of an electric current, as a satisfactory standard. If we are to use any incandescent surface there are two conditions that we must satisfy. In the first case we must have a surface which always, for a given temperature, emits radiations exactly the same, both in quality and quantity, and, secondly, we must make sure that the temperature of the surface which is radiating light to us remains the same throughout our observations. Those are the two conditions that I think Prof. Fleming lays down, and which have to be satisfied. I take it that the first condition can be satisfied satisfactorily if we use platinum or one of the metals of the platinum group. Mr. Petavel has recently been making experiments with iridium, platinum, and some other metals of that group with, I think, more satisfactory results than with pure platinum. I notice, however, in the last number of the *Thätigkeit* of the Reichsanstalt, an interesting account is given of the method which a carbon surface is raised by electricity to a high temperature to serve as a standard source of radiation (it is described in *Engineering* of October 31st last).

With regard to the second condition, there are various ways we may use to determine whether the temperature of the surface which we are employing as our radiator is definite in amount when we are employing it. One of these is the method which has been referred to in the paper by Dr. Fleming, the method of Lummer and Kurlbaum. It is well known that the proportion of light absorbed by any given material of a given thickness (say a cell of water two centimetres in thickness) varies with the temperature of the radiating source. Therefore if you can raise the temperature of your radiating source until a certain definite proportion of its radiation is absorbed, you know that the temperature of that source is definite. Of course, in realising that, which is what Lummer and Kurlbaum have done, you have the difficulty of measuring a 10 per cent. absorption, of making sure that the two sources of radiation which you are comparing in your apparatus differ by 10 per cent. There are, however, other ways

of realising the same result. The intensity of radiation differs in different parts of the spectrum, and the curves of radiation for the red and the violet parts of the spectrum are different. So that if you separate the radiated light from your source into two parts, by passing it through a spectroscope or otherwise, and if you isolate the radiations from the red and from the violet ends of the spectrum thus formed, and examine how they vary with the temperature, you will find that while for low temperatures of the radiating body the radiation from the red end of the spectrum is the greater, yet as the temperature of the source increases the radiations from the violet end of the spectrum become the greater, and for one definite temperature of the source the radiations from the violet end have the same intensity as those from the red end of the spectrum. The two curves which give you the relation between radiation and temperature cross at one point, and that point gives you a definite Dr. Glazebrook.

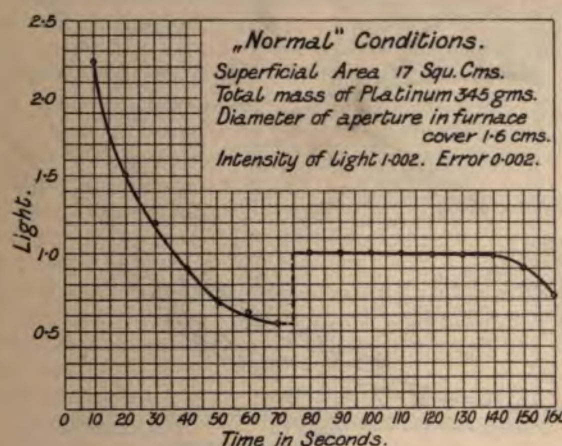


FIG. A.

temperature for your radiating source. That is a method which has been used by various investigators, and is used now at the Reichsanstalt. The objection to that method is that the two curves of radiation are very steep to the vertical axis, so that the point at which they cross cannot be determined with very great accuracy. But there is another method which can be used, the method which has been used lately by Mr. Petavel, and by means of Mr. Petavel's slides, which I will throw on the screen; I can explain the matter to you in a few moments.

Fig. A illustrates the normal results of an experiment on the intensity of the radiation from incandescent platinum. The curve gives the intensity of the illumination at intervals of ten seconds from a mass of incandescent platinum near its melting-point. I need not go into the details of how the curve is obtained, but you will see that starting ten seconds or so after the experiment begun there was a considerable luminosity, nearly $2\frac{1}{2}$, in certain units. That luminosity

Dr. Glazebrook. dropped very rapidly, and after 75 seconds had elapsed it suddenly rose again through a number of units from 0·5 to 1. At that moment the incandescent platinum began to solidify and then for some little time afterwards, from 75 seconds to perhaps 120 seconds, the intensity of the illumination remains practically constant and at what he calls the one unit. But in order to get that you have to repeat very carefully and exactly the conditions under which Mr. Petavel worked; this curve shows the result of one of his most successful and satisfactory experiments.

Fig. B gives the results of experiments on the comparison by means of two thermopiles of the light from the red and blue ends of the

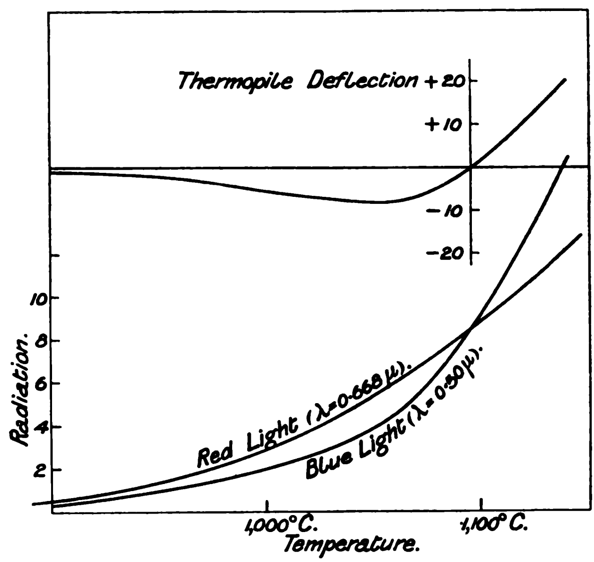


FIG. B.

spectrum. The light from the red end falls on one thermopile, and the light from the blue end falls on the other. The two thermopiles are connected in opposite directions through a galvanometer, so that its deflection measures the difference in the intensity of the two radiations. The red light alone causes a deflection below the zero line, and the blue one above. The resultant thermopile deflection is shown in the top curve. At first, in consequence of the fact that the red light is the stronger, there is a deflection downwards. The deflection increases and then gradually decreases, and when you come to the temperature of just under 1,100° C. the galvanometer deflection is zero and the radiation for the two ends is the same. That marks a definite temperature of the radiating source, and the total radiation emitted when this temperature is reached might be taken as your standard of light.

Dr. Glazebrook.

The horizontal distances (Fig. C) give the scale of temperature, 1,000° C., 1,100° C., and so on, and the vertical ordinates give the percentage of radiation that is transmitted by various thicknesses of water, benzene quartz, glass, and other substances. You will notice in all cases that as the temperature rises from 1,000° C. to 1,700° C. in those various curves, the percentage of light transmitted increases.

Fig. D gives exactly the same curves for some other materials. The percentage of light transmitted as the temperature rises is shown as increasing in the case of all substances except one light, black fluorspar, the curve for which comes downwards across the diagram in the opposite direction to that of the others. So that if you cause the light from a radiating body to be divided into halves and transmit one half through water and the other half through black fluorspar, and if you allow each half to fall on a separate thermopile, then as the temperature rises the thermopile connected with the water will rise in temperature, that connected with the black fluorspar will fall in temperature; and there will be a definite temperature of the radiating body, dependent on the thickness of the water and of the black fluorspar—1,360° C. in that diagram—at which the radiation from the black fluorspar is equal to the radiation from the water. If we can determine that, then it will enable us to keep our radiating body at a definite temperature, and then if we measure the limit emitted at that temperature we can treat the source as a standard of radiation.

FIG. C.

In Fig. E the arrangement of apparatus is shown diagramatically. To the left is the radiating body R. The source of light is split into three portions. The central portion passes on to A, and can be compared against a standard candle, the pentane lamp, or whatever is suggested. In the upper part of the diagram the stream of light is allowed to fall through a screen of water on to a thermopile P 1. The lower half of the diagram shows the light falling through a screen of black fluorspar on to the thermopile P 2. Those two thermopiles are connected oppositely through the galvanometer G., and you know that so long as the needle of the galvanometer is at zero the temperature of

your radiating source is definite in value. That is a temperature which with the same piece of apparatus can always be reproduced, and such a radiating body radiating at that definite temperature can be taken as the primary source of radiation. Such an arrangement seems to me, as far as I have studied the subject, to hold out very considerable hopes for success, and to open the possibility of obtaining a primary standard of radiation which shall be superior to the original Violle standard, and, I hope, in many ways to the Lummer-Kurlbaum standard used in Germany. I think Mr. Petavel deserves great credit for his work.

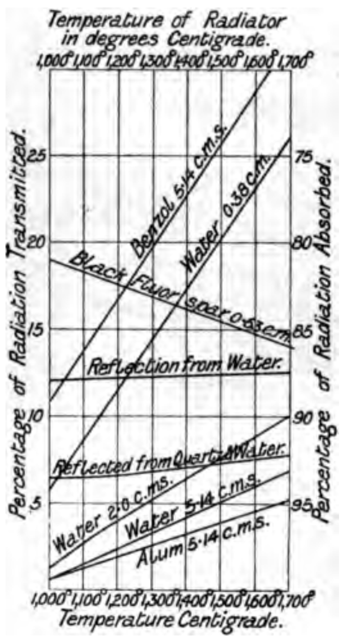


FIG. D. (*see p.* 177).

There are many points in the paper to which I should like to refer, but I think I have said all I specially wish to say by calling your attention to this recent work of Mr. Petavel's, which was laid before the British Association at Belfast, and which has not, I think, been noticed very fully in the scientific Press.

Sir WILLIAM DE W. ABNEY: I will only deal with that part of the paper with which I am most accustomed, namely, what we may call colour photometry. The various methods which have been shown to us, such as the flicker photometer and another photometer whose name I forget, do not, I think, measure the illumination. They go a degree towards it, but they do not measure the brightness of the light. The only way in which you can measure the brightness of the light is to place the colours side by side and judge when they are of equal brightness. That is the method which I have been accustomed to use for a great many years now, but apparently it is not as well known as it might be. It is an excessively simple method. A red and green light can be matched together just as readily as two white lights of the same colour can be, provided you use the oscillation method. The error of observation becomes very small with practice. It may be asked what the proof is that what you are measuring is the brightness (or luminosity) and not something else. I will prove it in this way. Supposing I wish to compare the brightness of two spectrum colours, one, say, in the red near the place of the red lithium line, and the other a green near the magnesium line. Slits placed in the spectrum at those particular parts will pass rays which, combined together, will form white. By opening or narrowing the slits a patch of white light can be thrown on a screen by my colour patch apparatus which cannot be distinguished from the white of the comparison light.

I may say that I use the crater of the positive pole of the arc light for the light to form the spectrum, and I use the whiteness of that light for the purpose of comparison. The mixed rays forming white on the screen, a patch of white light from the comparison light is thrown over it, and a rod, as by the ordinary Rumford method, casts two shadows side by side. By means of sectors the shadows are equalised, and the angle of the sectors read off. The green light is covered up, and we have a red alongside the white. Again, by means of sectors, which have movable apertures whilst rotating, the white is made brighter or darker alternately than the red. There is some point between those two in which the white is neither too light or too dark in comparison with the red, and by gradually diminishing the oscillations you come to a point in which there is no flicker whatever, no winking. The two shadows seem to wink at you after you pass the neutral point, but there Sir Wm. de Abney.

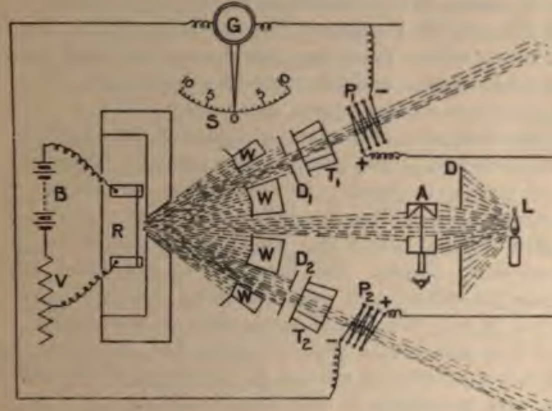


FIG. E. (*see p.* 177).

is a point where the winking ceases altogether, and this is the point where the sector shows equal brightness. You read the sector off, and you say you have got so much—30°, say, of the white. The red is shut off and the green is placed against the white, a measure is again made, and we get a certain angle, say another 30°. If the two happen to be 30° each, you will find that the white light is 60°. You may go right through the spectrum in the same way, and you will find that is the case—the sum of the measures of all the different components is equal to the measure of the lights combined. That is a very important point. If it does not mean the measurement of brightness, what does it mean?

There is one other point on which I should like to remark, and that is the question of white light. Everybody looks askance at you when that question is put. There is, however, a physiological white light which has a very definite whiteness. I do not know whether the audience is aware that they are all colour blind at parts of their retina, but it is a fact. Everybody is more or less colour blind. I do *not say that they are colour blind* through the whole of the retina,

ʼm. de y. but there are portions of the eye which are absolutely colour blind; they can see no colour whatever. If I had a spectrum apparatus I could show it in a minute. If you take a spot of green light and look at it with the centre of the eye, you will see that it is green, but as you move your head, and your eye with your head, you come to a point at which you see no colour whatever on the part of the retina on which you receive the image; the green has gone, and you see a pure white light. Whatever colour you take, that colour will always give the same quality of white light throughout. This is what I call the physiological white light. The white light of the crater of the positive pole is very nearly uniform with that, as we can tell by comparing the two. My standard of white light is what I call the physiological white light.

To go back to the subject of photometry, Prof. Fleming mentioned Purkinje's phenomena. That is a phenomenon which was measured qualitatively by him, but I think if he will refer to a paper which I had the honour of reading before the Royal Society a good many years ago, he will find not only the Purkinje's phenomena set forth, but the absolute details as to where every ray commences to bend and where it takes its normal course and becomes a straight line with different intensities. There is a point where every ray becomes a straight line; the starting-point or zero of the red is different from that of the blue, and the blue from that of the green. The stimulus required to give a sensation of colour is greater in some than in others. The zero points are not the same. But this difference in zero points only need be taken into account when the lights to be measured are, comparatively speaking, feeble. You have not to pay great attention to that phenomenon when you are measuring lights of the ordinary brightness; it is only when you come to low luminosities that you need take it into account at all. The fact that you have moonlight in which there is practically no red is one effect of the difference in zero points, and moonlight is only dim sunlight.

There are one or two other points that I should like to mention. This method of distinguishing lines is not a method of measuring the brightness at all; what is measured is the acuteness of vision in some particular colour. Acuteness of vision is a very different thing to brightness. The acuteness of vision in the yellow is very much greater than the acuteness of vision in the blue. Of course there are reasons for it. The accommodation of the eye is different for the blue from what it is for the yellow or for the red. One of the supposed measurements of luminosity of the different rays of the spectrum was published by Professor Langley of the United States. He obtained his measures by reading logarithmic tables in the spectral colours. When he could no longer see what the figures were on a particular page of logarithms by dimming the light, he noted the point. He went through the whole of the spectrum in this way, and from it plotted a luminosity curve of the spectrum. What he had plotted was really an "acuteness of vision" curve. The acuteness of vision curve which was derived from the log readings would also be derived from noting the disappearance of the ruled lines of different fineness which the author employs. I am endeavouring to indicate

some of the difficulties we have in colour photometry generally. I am not talking about the ordinary intensity photometry, but only where you have delicate photometry to do. I do not suppose anybody has realised the fact that if you have a white paper spot an inch in diameter, and another a ¼ inch in diameter, and place them on a black background, say, three or four inches apart, the big inch spot is considerably brighter than the ¼-inch spot. Although you have cut them out of the same paper they will be of a different brightness. If you take colours the same thing happens. Supposing you have two patches of spectrum colour of red ¼ inch in diameter and another an inch in diameter. If you stand three or four feet off the brightness is different. It shows that when you are dealing with photometry of a delicate nature you have to take into account all the phenomena and all the deficiencies which exist in the eye. It may be said regarding the two white spots that the yellow spot of the eye would account for that occurrence. It might in a certain sense, but it will not account for the difference in intensity of the red spots, because the yellow spot of the eye allows all the rest of the red to go through without any absorption whatever. The flicker photometer comes under the same ban, to my mind, as the other. Flicker photometry is based on a physiological fact, the persistence of vision after the stimulus has been removed, and it does not give the same curve as the ordinary brightness test. Prof. Draper of New York, I think, was the first to measure coloured light in that way. I was in commuication with him at the time he was making those observations and when he published them. After comparing them with the other methods one came to the conclusion that he was measuring some form of acuteness of vision. I think you have to be very cautious to show that acuteness of vision is equivalent to brightness of light. What is the difference between gas-light and electric light? You may say the one is whiter than the other. No doubt that is quite true, but do you know how much that whiteness is due to the blue? The blue sensation of the electric light is about $\frac{1}{100}$th part of the red sensation in the arc electric light; you may cut off the blue sensation if you like, and it will give you a difference in luminosity of about $\frac{1}{200}$th part of the whole, so that you can very nearly reduce the arc light to the colour of an incandescent light by suitable means and without very greatly damaging your measurements.

Sir Wm. de Abney.

The subject of the paper to-night is very wide; and when one has lived among photometers for so long as I have, one has much to say. There is another method by which very accurate methods of ascertaining the brightness of a light can be obtained, and that is by extinction. If you have a suitable absorption apparatus and a suitable medium you can totally extinguish the light reflected from a surface, though radiation is still passing, but of course proper precautions must be taken as to how you use your eyes. You have to keep in the dark for some considerable time, and by that means you can get a very good measure of the brightness of any light by knowing the amount of absorption that has to take place in order to extinguish it. That was one of the first methods I adopted in some War Office experiments undertaken some thirty years ago when the

Wm. de ley. electric light was being introduced into the service. I was then charged with this branch of work at Chatham, and I was called upon to measure the intensity of those lights. At that time I came to the conclusion that the extinction method was perhaps the best method extant. I do not say it is the best method now, but at that time I had the available apparatus, and I measured the lights by that means with very great ease. Finally, let me say I do not see the slightest difficulty in comparing even, let us say, a smoky railway lamp with the arc light; it is simply a matter of practice. If you have been doing the work for some time you never, or very rarely, make a mistake. Anybody who has worked at colour photometry seldom fails in measuring correctly lights of totally different colours. I prefer myself to have a neutral comparison light (a colour intermediate between the two taken to be compared) for getting the closest readings. White for general work is a neutral colour, and that is why it should as a rule be used.

sident. The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected :—

Member.

Arthur George Way.

Associate Member.

Geo. Frederick Alexander Norman.

Associates.

Horace Bourne.
Frederick Bruce.
Francis James Humphrey.
Robert F. Morris.
Fitzroy Owen Jonathan Roose.
J. Charles Serjeant.

Student.

Maurice George Tweedie.

The Three Hundred and Eighty-fourth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, December 18th, 1902—Mr. JAMES SWINBURNE, President, in the chair.

The minutes of the Ordinary General Meeting held on December 11th, 1902, were read and confirmed.

The names of the new candidates for election into the Institution were announced, and it was ordered that these names should be suspended in the Library.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Associate Members—

Thomas W. Bloxam.	William Sillery.
T. W. Broadbent.	Alan Smout.
Archibald Campbell.	Frederick Soloman Spiers.
Wm. Leonard Carter.	Ernest F. Szlumper.
Thomas Cooper.	Edward Ernest Tasker.
Lieut.-Col. J. H. Cowan, R.E.	Robert Tervet.
Arthur Charles Devey.	Frank Charles Thomas.
Edward Dixon.	G. Thomas-Davies.
Edward G. Fishenden.	Herbert E. C. Tutte.
Urban B. Gilbert.	Frederick M. Walker.
John Geo. Holdsworth.	William Walker.
Arthur Llewellyn Lean.	Frank Wallis.
Henry Luttrell-Elward.	Charles T. Walrond.
Joseph Roper Penning.	John A. Walter Ward.
Frank C. Porte.	Burkewood Welbourn.
George Robertson, Jun.	Charles F. Wilkins.
J. M. Shackleton.	C. H. C. Woodhouse.
Henry George Shoolbred.	Ernest H. Wright.

John Ortelli Zerega.

Messrs. C. W. Barnes and R. W. Hughman were appointed scrutineers of the ballot for the election of new members.

RESUMED DISCUSSION ON PAPER ON "THE PHOTOMETRY OF ELECTRIC LAMPS" BY DR. J. A. FLEMING, M.A., F.R.S., MEMBER.

Mr. A. P. TROTTER : The paper before us is, I think, the first we have had in this Institution on the question of Photometry. We had a paper on A Photometer, the discussion upon which turned largely upon the abused and moribund Standard Candle. The subject of photometry has occupied the attention of a very few other scientific Mr. Trotter

Trotter. societies, but there was one most important Physical Society paper by Prof. Ayrton and Mr. Medley in 1895, which was the first publication on the rise and subsequent fall of candle power of glow-lamps as they age. Beyond that very little indeed has been done, but the subject is, I think, very well worthy of attention, considering the large proportion of the electrical industry which is employed in simply producing light.

The first part of the paper deals with the standards of light. If I criticise the pentane lamp, it is purely from the point of view of the electrical engineer. I should have liked to hear, and perhaps we may still hear, the opinions of the gas referees upon the subject. At all events, I would like to know whether the various errors to which this lamp is subject cancel out when a gas flame is compared with it; I imagine they do. I suppose gas engineers do not care very much about the errors due to carbon dioxide in the air, or to the humidity. But there is a rather important variation, something to be corrected for, to which Dr. Glazebrook alluded on the last occasion, that due to the barometric pressure. It was very evident that a lamp depending for its supply of fuel on the gravity of the heavy vapour falling, must vary a good deal with the barometric pressure, and the variation is something like four and a half times as much as that of the Hefner lamp. The Hefner lamp, on the other hand, has met with considerable criticism from Dr. Fleming, but I think it is not quite so bad as he suggested. I used this lamp in photometry with which I occupied myself nearly ten years ago, and I consider it to be a very useful and practical standard, its most serious disadvantage perhaps being its colour. Dr. Fleming, perhaps, is not quite logical when at one moment he said the colour was so bad that it was impossible to use it, and a little later said the Flicker Photometer so completely got over the colour difficulty that he could easily measure an arc lamp against a candle. If that is the case, the objection disappears. I have brought down my lamp to show you. The mode of measuring the height of the flame is rather different from the one shown by Dr. Fleming. This has the Krüss optical flame gauge; it is a little camera, with a lens and a ground glass screen, and on it is a line for the standard height of the flame, 40 mm. There is a gauge supplied with it for gauging the height of the top of the wick-holder, and a point 40 mm. above it. The only difficulty I found was with regard to the purity of the amyl acetate. Certainly it will not do to buy the amyl acetate at any chemist's shop any more than you can buy the pentane at any chemist's shop. It is best to buy it from the makers of the lamp, but I find that even the official quality corrodes the brass work a good deal, so I added a little tap to run it out when the work is over. It is a good thing to wash the lamp out with alcohol before putting it away. The flame has a singularly low temperature. I suppose this accounts for the reddish colour, and for the small draught. The flame is therefore easily disturbed and must be protected from draughts.

On pp. 129, 130 some formulæ are given—rather foggy formulæ, I am afraid. The first one deals with the quantity of water vapour. That *need not concern* us very much. The wet and dry bulb thermometer

is good enough, but the variations are not very great and can be allowed for approximately. The next one is the carbon dioxide in the air. That is practically negligible in the ordinary laboratory. Dr. Fleming mentions how it was noticed when a good many students came into a small gallery where a 10-candle pentane lamp was being used; but you do not get much carbonic acid gas in an ordinary ventilated room, using electric lamps and a Hefner lamp. Liebenthal's formula $L = 1{\cdot}012 - 0{\cdot}0072\,x$, where x is litres of carbonic oxide per cubic metre, assumes a unit value for the light when these are 1·66 litres of carbonic oxide per cubic metre, or in plain English 16·6 parts in 10,000, or in still plainer English an extremely stuffy and unhealthy state of the air. One of the most important of the formulæ is the one relating to the height of the flame. I will put another formula of the same class on the board— Mr. Trotter.

$$P = \Big(18 + 1{\cdot}5\ (n - 12)\Big).$$

P is the price in shillings per dozen of lamps: the formula simply means eighteen shillings a dozen. I say that that is not the right sort of thing to offer to engineers, although it may do for the laboratory. I do not blame Dr. Fleming for contriving it, only for reproducing it. Dr. Liebenthal is responsible for it. The long and the short of that very foggy formula is, that the height of the flame is proportional to its candle-power when it is over 40 mm. I attach great value to Liebenthal's work on the Hefner lamp, but, nevertheless, I say that it is an abuse of mathematics to give us a formula of this sort.

The height of the flame which will give us one candle-power is rather an important matter, and Dr. Fleming gives 0·88 as the multiplier, being the average of a number of measurements. I have used 0·877, the average of a different set, but I always think of the relation as a difference of 14 per cent. and not as a fractional multiplier. Dr. Fleming's figure works out at 13·6 per cent. I think that until an authoritative comparison is declared, 14 per cent. is as close to the mark as 13·6 per cent. On this little ground-glass screen I have drawn another line $5\frac{1}{2}$ mm. below the official black line, and this gives me the height of the inverted image the flame which corresponds to one British standard candle for all practical purposes. The Standard Candle is now becoming an arbitrary unit embodied in the pentane lamp, and this, whatever be its defects or merits, must be accepted of course as the official 10 Standard Candles, and we shall base any standard lamps upon that and not upon the measurements with actual candles, which have had their day. This formula is an important one; Dr. Fleming gives it correctly, but I think this is a good opportunity to call attention to the fact that in Palaz's well-known book on photometry it is given with two misprints, and these are reproduced in the American translation of Palaz's work. I did not read Palaz until after I had stopped my photometric work, and I used to work with the 14 per cent. Palaz does not quote from the original paper, but from *La Lumière Électrique*.

Dr. Fleming's standard lamps with aged filaments put into new globes

Mr. Trotter. is certainly an admirable idea. But there is one point about them which does not satisfy me, and that is the use of a clear-glass globe. I have found, in using a glow-lamp as a working standard, that it is extremely important to have either a ground-glass bulb to the lamp or a ground-glass shade; because not only do very small irregularities in the glass produce very considerable refractive effects, and the very smallest movement of the lamp makes a very considerable difference if you are dealing with anything like 1 or 2 per cent., but you have an imperfect concave reflector at the back of the lamp which has no particular focus. I do not think it is sufficient to say you should use a lamp in a definite direction. I have always used a ground-glass lamp, taking care to handle it by a flange attached for the purpose to the socket. I think perhaps a ground-glass cylinder which can be cleaned carefully with ether to make it perfectly clean, is better still, and if it is regarded as a part of the standard no correction need be allowed for the absorption of it.

The photometer to which Dr. Fleming is most attached is the Lummer-Brodhun, which calls itself in the advertisements a precision photometer. I rather object to any photometer calling itself a precision photometer. It has the disadvantage to many English people that you have to put your eye against an eye-piece. A German scientific man is never happy unless he has his eye glued against an eye-piece, and I think Sir William Abney agrees with me that in photometry you want to take a general good view at a thing, standing some way off, and forming a judgment, because after all photometry is almost entirely a question of judgment. Photometry is not a physical measurement; it is a judgment, and you must base your physical measurement indirectly upon the judgment of the eye. Dr. Fleming also alludes to various other photometers. We do not hear much about the Bunsen photometer now. Many text-books on physics make the extraordinary error of thinking that a disappearance on a Bunsen screen means a balance. Mr. Stine, in a very good American book on photometry, gives a picture of the Bunsen photometer, with a plain disk erected on a bar with no mirrors. It is perfectly useless to put up a Bunsen screen, and to get the disappearance, and then say you have a balance. If you look on the other side you find the appearance is quite different. It is essential, in work with any photometer of the Bunsen type, to have two mirrors and regard the spot from the same angle by means of the mirrors.

In the next place Dr. Fleming goes in for the consideration of the dark-room and recommends an elaborate blacking of the photometer room. But I have found, working in an ordinary room, that you can do a good deal of accurate photometry by using black cardboard screens with holes in them, on the principle of the rifle range, of course putting your light in a black box. In my photometry work my most valuable assistant was a little piece of looking-glass. If I wanted to know whether there was any stray light falling on the photometer I screened the direct light with a small screen and placed the looking-glass so that I could look into it at an angle, and get, as it were, right into the photometer, looking from the photometer's point

of view to see if there was any stray light about. If there was not, I was satisfied that the effect was as good as if I had painted all the walls of my room black. Mr. Trotter.

Dr. Fleming suggested a method of measuring the candle-power of an arc lamp which is not unlike the one described by Mr. Weekes in a discussion on a paper which I had the honour of reading before this Institution in 1892 on the candle-power of arc lamps. Mr. Weekes, like Dr. Fleming, took the horizontal beam as the standard. I think that is the worst beam to take, because with the smallest inaccuracy in the shape of the crater, or if it is a little bit on one side, the strength of the beam must be altered very seriously. The best beam to take would be somewhere about 45°, or where you get a full view of the crater. I have never tried the horizonal beam, but I say now, as I said in my criticism of Mr. Weekes' remarks in 1892, that it does not appear that the horizontal beam is a good one to take as a unit for comparison. I am very relieved to find a record of experiments showing the proportionality of the moving sector of a Fox-Talbot disk, because an American some eight years ago published a paper in the *Physical Review* making out that it was quite erroneous, and that the revolving sector did not cut off the simple proportion of the light, and that some physiological effect took place. It is a great comfort to find these results, because the bulk of Sir William Abney's work would have been valueless if it were true, and we are perfectly certain it is not, because Sir William would have been utterly confused years ago if the American suggestion had been true. Dr. Fleming goes on to discuss the polar curve of a lamp. I am very glad to see that in print in the Journal, because in my paper to which I have referred I made a bad mistake, and this puts it right. Dr. Fleming then goes on to say that before making experiments predetermined calculations of the illuminations of arc lamps should be made. I think that has been rather over-done already. Palaz's book is full of it; Blondel has done a lot of it; Maréchal, in his book on the lighting of Paris, has also investigated the matter, and I, in my paper read before the Civil Engineers in 1892, contributed a great many. We have been over-burdened with pre-determinations. What we want to do is to get actual measurements, of which I have done a few myself. Pre-determinations are interesting to calculate and plot, but as a matter of fact when you come to compare them with practice M. Blondel says they do not at all agree with the results. I concur, although I understand that M. Blondel made this remark in support of pre-determinations and in disparagement of plotted results of actual measurements. I will say, in conclusion, that I disagree with the objections to the candle and lux as units. The quantity 0·05 of a lux is a most important one in street work. The lux-second is an extremely important thing in photography—in fact, though hardly any one realises it, the whole question of photographic exposures is based upon the lux-second. All the arbitary plate numbers and constants of the Watkin and Wynne actinometers ought to be expressed in lux-seconds. It was "adopted" by the International Photographic Congress at *Brussels in 1891*, but so far as I know, is never used.

Mr. Trotter. I see no reason for disturbing the order of magnitude of the unit of luminous intensity. It is an effort to use this expression—luminous intensity, one would naturally say—candle-power. The expression candle-power is firmly fixed; nobody is going to talk of luminous intensity except in the lecture-room. To speak of a candle-power of one "lamp," meaning a candle-power of ten old candles, would be very confusing.

My appreciation of the Hefner lamp as an instrument does not bias me in favour of the Hefner unit as compared with the Standard Candle. Personally, I should be content to take the National Physical Laboratory declared value of the ten-candle pentane standard as ten British candles and use standardised glow-lamps or a Hefner lamp, with the National Physical Laboratory factor of 14 per cent. or whatever it is found to be. But the Hefner unit is established in Germany, and has

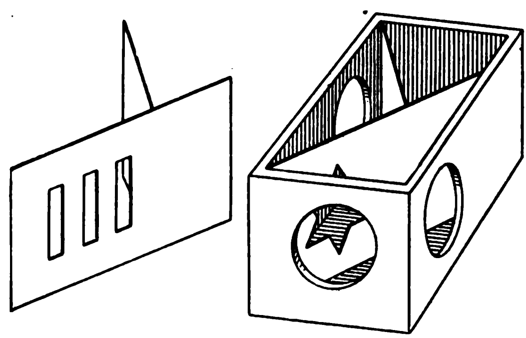


FIG. F.—Photometer box with star hole; and screen shown separately with slot holes. For the sake of clearness the holes are shown larger than in practice.

been accepted in the United States. To give up the old "parliamentary candle" and to adopt a new value would cause but little confusion and expense; and the old candle is not a standard of which we are proud. The arguments against abandoning or shortening the yard, and for adopting the metre instead, do not apply in this case. We cannot expect other nations to adopt the British candle. Lamp-makers ought to welcome the change, for 14 candle-power lamps will become 16 candle-power and 16 candle-power will become 18¼ candle-power.

A photometer which I described in a paper before the Physical Society in June, 1893, does not seem to be known. It achieves the result aimed at by the Lummer-Brodhun photometer in a much more simple way. In the Lummer-Brodhun instrument you see two images of the opposite sides of a screen. One image appears to have a hole in it, and you see the other image through this hole. To arrive at this result you have to use four prisms or two mirrors and two prisms. The prisms appear to be the latest improvement. These prisms have no less than eleven surfaces which must be truly plane and clean, and besides these there are one or two lenses. My photometer is

nothing more than a modification of Sir John Conroy's or Ritchie's photometer, and the modification is in the same direction as that made by Prof. S. P. Thompson. There are two screens set at the same angle to the light; one has a hole or holes in it, and you look at the further screen through the holes in the nearer screen. The angle of 45° alluded to by Dr. Fleming in various photometers is the worst angle, for if there is any glaze it will cause trouble, and to avoid all glaze is very troublesome. The best angle is about 35°. Not only may this angle be varied a little in either direction without causing much difference in the brightness, but the brightness is greater than at 45°. I see no object in using compressed magnesia. What if it be brighter by one or two per cent.? Shortening the photometer bar by an inch or two would more than do that. I have tried various materials, including screens painted with magnesia white-wash. I find that good Bristol board (white cardboard), with the glaze removed by a damp cloth or by pumice powder, is excellent. If one hole is used it may be star-shaped, distorting the star so that when seen at an angle it appears symmetrical. The edges must be carefully bevelled. Another form of screen has several slots which are used on the principle of limit gauges. Mr. Trotter.

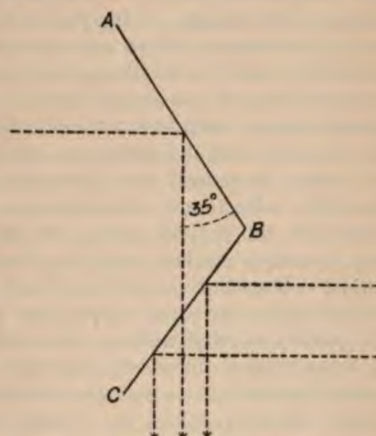


FIG. G.—Plan showing rays from the left striking the screen A B, part of which is visible through a hole or holes in the screen B C, which is illuminated from the right.

When the middle slot shows a balance the slot on one side is brighter and on the other side less bright. The back of the front screen should be blackened to avoid reflected light. The whole arrangement should be reversible to prove its symmetry, or to allow a mean to be taken. The instrument shown was made by Messrs. Nalder Bros. The method of double weighing recommended by Dr. Fleming, to get over want of symmetry in the photometer, is a poor comment on its construction.

In conclusion I offer four additions[1] to the valuable bibliography collected by Dr. Fleming.

Mr. KENELM EDGCUMBE: It seems to me that the paper to which we have just listened is too good, too excellent a paper; that, in fact, it is rather disheartening to any of us who are rash enough to want to measure the candle-power of, say, our incandescent lamps. What are we to do if we have not such a palatial apartment as Dr. Fleming would have us set aside for a photometer room? Personally, I have Mr. Edgcumbe.

[1] [These are now incorporated in Dr. Fleming's Bibliography.—ED.]

Mr. Edgcumbe.

often had to work with the ordinary enclosed photometer, and I am very pleased to hear Mr. Trotter say that in his opinion good results can be got with one. I have certainly found the screen mentioned by Mr. Trotter to be most satisfactory; and this type of photometer, which I might perhaps call "the hole-and-corner photometer"—that is to say, a shelf, with a curtain in front of it—is certainly all the bulk of us can ever use. It has, moreover, several advantages over the photometer room. In the first place, we can read the divisions on the scale without any chance of seeing the lamps themselves, which is important; and, moreover, the instruments can be easily read.

In regard to one of the diagrams (Fig. 5), I should like to hear how Dr. Fleming proposes to regulate the voltage on what he calls the "comparison lamp." I take it, he uses another resistance, and measures the voltage on the potentiometer. This is a perfect arrangement if you have a 100- or 200-volt battery at your disposal, and which is not being used for anything else; but it is quite out of the question when working off the supply mains. For instance, you measure the voltage on one lamp to, say, one-hundredth of a volt, but by the time you have adjusted the other one, the voltage has gone down four or five volts. Moreover, for alternating currents, which are extremely useful for adjustment, and also for obtaining various voltages, you cannot use the potentiometer. If, on the other hand, the standard lamp is simply marked as giving such and such a candle-power at a definite voltage, it is merely necessary to connect the two lamps up in parallel across the same supply, and adjust roughly by the voltmeter. We know then that we have the same voltage on each lamp, and as it has been repeatedly shown that the candle-power bears a perfectly definite relation to the voltage, even with different types of lamps, it is quite immaterial how the voltage may vary, and the ordinary voltmeter is quite accurate enough for the purpose.

Dr. Fleming says that, in his experience, no voltmeter or ammeter is sufficiently accurate for photometer work. If used in the way he suggests, namely, by adjusting separately, I quite agree with him as regards the voltmeter, but as for the ammeter, I think it is amply sufficient, as a half per cent. or 1 per cent. error is quite negligible in photometer work.

I have lately been trying to do what, I fear, Dr. Fleming would consider impossible, namely, to measure the candle-power of ordinary incandescent lamps without any photometer room at all. I have placed on the table the instrument as it is at present constructed. It consists, as will be seen, simply of a box with two removable ends. One end is to be taken off, and the other has a hole cut in it. In the middle is a partition with the photometer proper—a grease spot. Stretched out to one side there is a tape, divided off directly into candle-power. In order to standardise the apparatus, you take the standard incandescent lamp, which gives, let us say, 16½ candle-power at 100 volts, put it on the mark representing 16½ candle-power on the scale, and move another lamp on the other side of the screen until you get a balance. Both lamps are run in parallel, and the instrument then becomes *direct reading*, it being merely necessary to replace the standard lamp

by the lamp to be tested. It has the advantage that it can be used anywhere—in this room, for instance. Stray light does not affect the accuracy of the apparatus, because when you are making the first comparison the light is there, and you are balancing with it. Mr. Edgcumbe

Mr. J. T. MORRIS: I propose to confine my remarks to the first portion of the paper, which deals with standards. Dr. Fleming divides these into two groups—primary standards and working standards. With regard to the former, having had the pleasure of seeing some of Mr. Petavel's work on the Violle standard, I can say that the colour of that light is very suitable for photometric work. In the amyl-acetate lamp, on the other hand, I consider that the colour difficulty does render accurate photometry more difficult, and for the primary standard of light one would naturally select one whose colour nearly resembles that of the light which one usually has to test. Mr. Morris.

Then, with regard to the setting up of the Violle standard, a large mass of platinum is required. Dr. Fleming mentions Mr. Petavel's statement that half a kilogramme is required. But that is a matter of over a pound of an extremely expensive metal. Further, the standard is one which, in the way Mr. Petavel used it, was only transient. The platinum was heated by means of an oxy-hydrogen flame to a high temperature, then the arrangement was allowed to cool steadily, and when it came to the temperature of solidification, there was a delay in the steady diminution of light from the platinum which lasted, in his case, for a period of about fifty seconds. Now, fifty seconds is hardly long enough for a primary standard of light to last. I would therefore suggest that it would be better, if such a standard is to be employed, to use electric heating by means of alternating currents. So long as one is dealing with secondary batteries there is considerable difficulty in connecting them in parallel for suitable working. But if an electric welding transformer were used for the purpose, I imagine more successful results could be obtained, only the current should not be cut off completely from the platinum. After the platinum is thoroughly melted the current should be reduced to such an amount that the platinum will, in time, completely solidify, and this will give a much longer time over which one can make observations of the actual light which is emitted during solidification.

Then, passing to the matter of working standards—and, I believe, by working standards Dr. Fleming means standards which are capable of something like one to a half of 1 per cent. degree of accuracy — I have had little practical experience with the amyl-acetate lamp.

With a simpler form, however, of the Vernon-Harcourt lamp, known as the Simmance pentane standard lamp, I have worked and found it extremely useful in the laboratory. The chief advantages of this lamp are that it quickly attains its normal candle-power, and also that it has no chimney surrounding the flame. The advantage of its having no chimney is that it does not take a long time to heat up. This was a very serious objection to the Woodhouse and Rawson form of the lamp, which certainly took half an hour before the lamp gave its normal amount of light.

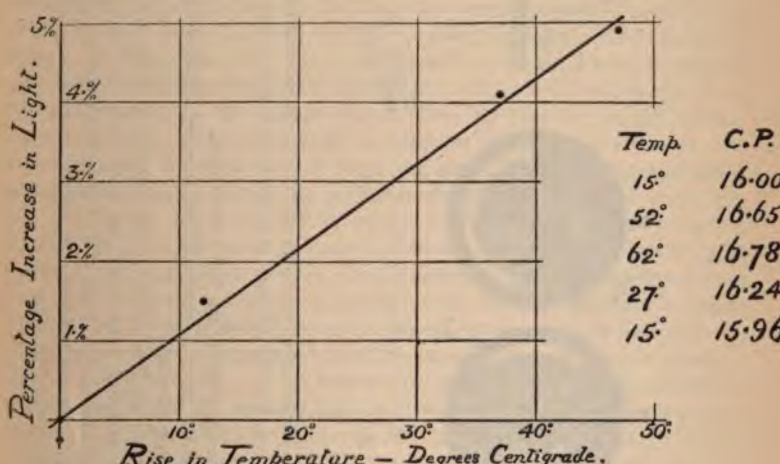
Turning *to incandescent lamps,* when an incandescent lamp is to be

Mr. Morris. used as the working standard, we must first of all discard all filaments which have more than one loop in them. The horse shoe filament is a convenient one, because it is all in the same plane, and therefore one can tell accurately the distance that the filament is from the photometer with which one is working. I have had the pleasure of working for Dr. Fleming with some of these Fleming-Ediswan standard lamps some six years ago, and comparing the work that one can do with the ordinary horseshoe filament in the small and large bulbs, I consider that the large bulb form is much more satisfactory. Any slight change in the orientation of the bulb does not influence the candle power so seriously if the large bulb is used. Incandescent lamps of any kind have the advantage over flame standards that they are entirely unaffected by variations in carbonic acid, moisture, and barometric pressure; but, on the other hand, great care has to be taken in the measurement of the electric pressure. Mr. Edgcumbe has remarked that if your voltmeter gives you a result accurate to within half per cent. or 1 per cent. you get quite near enough results. That may be so in ordinary lamp photometry for works measurement, but if you want to obtain a degree of accuracy of 1 per cent. in the light, it is essential that the pressure be measured to one-sixth part of one per cent. [Mr. EDGCUMBE: I meant when the two lamps are in parallel; not when you are working against a primary standard.] Perhaps I have misunderstood you. However that may be, in any case where a single lamp is used for photometry, if you require an accuracy of half a per cent. in your candle power, you must measure your volts to $\frac{1}{12}$ or $\frac{1}{13}$ of 1 per cent. Measuring instruments are then practically out of the question, and a potentiometer, in some form or other, almost becomes a necessity. The filament that Dr. Fleming has shown is one of the horseshoe type. It might be a little more satisfactory if that filament could be held in a rigid position, and so be maintained in a perfect plane, if possible, because there is a tendency for it to wobble slightly under certain circumstances. But these are only minor criticisms, for I consider the large bulb lamp is certainly a very marked advance.

There is just one other point, and that is the regulation of the temperature of the photometer room. I should be glad to know if Dr. Fleming has carried out any experiments with regard to the relation of temperature and the candle-power of these lamps. Since the last meeting it occurred to me that I might put the matter roughly to the test, and I carried out the following experiment: I had two 55-volt 16 candle-power lamps connected in parallel so that they were run at exactly the same voltage. They had horseshoe filaments and were fixed six feet apart. A photometer was placed between and their candle-power was carefully compared. Then an arc lamp resistance which had been placed underneath one of the lamps was arranged so that it could be heated by means of an electric current and so the temperature of the space round one of the lamps could be varied, and in the curve the results are indicated. Along the base-line is plotted the rise in temperature of the space surrounding the heated incandescent lamp. That temperature was measured when the lamp was turned out. The

Mr. Morris.

candle-power is measured upwards, but in this diagram I have plotted the percentage increase of the light, not the absolute candle-power; so if one considers it as a matter of total candle-power, the base-line is one hundred times the width of one of these divisions down below. So we have percentage increase in light and rise in temperature. There were only four points obtained, after which the lamp was allowed to cool. The spot below the starting point of the curve indicates the nearness with which one can get back to the original value. The table accompanying the curve shows the relation of the actual temperature of the space surrounding the lamp when it is switched out, to its candle-power.



Temp.	C.P.
15°	16·00
52°	16·65
62°	16·78
27°	16·24
15°	15·96

FIG. H.

This experiment shows that in these particular 55-volt lamps for every 9° centigrade rise in temperature the light given out by the lamp rose 1 per cent.; so quite apart from the necessity of ventilating the photometer-room and keeping it cool for potentiometer work and flame work, it is also advisable to keep the temperature moderately constant so that the light given out by one of these working standards shall remain constant.

Mr. Varley

Mr. F. H. VARLEY: There are one or two points to which I should like to draw attention. The first is connected with the pentane lamp, to which Mr. Vernon Harcourt referred on the last occasion. Dr. Fleming gives two readings taken six years apart, in which a variation in the glow-lamp of about $\frac{1}{12}$ occurred in the six years; this he attributes to the fact that the pentane lamp was not reading at its full value, and hence the higher reading for the incandescent lamp. Referring to the air-gas pentane lamp, such a variation there is no doubt is not due to any fault of the pentane, nor to the lamp, but is entirely due to impurities in the air. The lamp draws in air through the valve, and is actuated by pentane vapour. If the air is moist, then

Mr. Varley. there is a repellant action, and it does not absorb so much pentane vapour as when the air is perfectly dry. We know the effect of moisture in the air as far as combustion is concerned: it cools the flame, and causes imperfect combustion of the carbon, and the flame

Fig. K.

I. The disks of the photometer showing the semicircular windows and their relative position on the disk. The second disk has precisely similar windows, but the window x' is reversed, being on the left.

II. Represents x'' fully open, x' being shut.

III. Shows the position of x' and x'' when two equal lights are being measured.

IV. The relative opening of the windows x' and x'' when a powerful light is being measured against the standard, say, electric arc light and a 10-candle lamp standard.

V. Bird's-eye view showing the equal distance of the shadow-receiving screen from arc light and standard candle; also, in section (indicated by parallel thick lines) the front disk, the back disk, a central axis attached to the former, a hollow axis attached to the back disk, f standard light, g light to be measured, x shadow-casting pin, ss' the shadows. Between f and g a suitable screen is provided, not shown in drawing, to prevent one light interfering with the other.

smokes. If precautions are taken to render the air perfectly dry, and the organic matters, chiefly ammonia and carbon dioxide, be removed, then having got a standardised pentane, a standardised burner, and a standardised atmosphere, not only to make the air-gas, but also to burn

it, one should have removed all the variations due to the pentane lamp, except those of barometric pressure and temperature. Those surely can be calibrated, and a constant given for each millimetre change of pressure, or each degree of temperature. To obtain standardised air, it should be drawn from an independent source free from the contaminated atmosphere of the photometer-room, passed over lumps of pumice-stone saturated with potassium permanganate solution to remove organic impurities, then through unslaked lime, and finally through calcium chloride to extract moisture and CO_2. Mr. Varley.

With regard to the question of photometry, in the year 1889 I devised a photometer which is a direct-reading photometer (Fig. K) and which has the advantage that it can be used in a very small space. I made these two cardboard disks to illustrate the principle of it. It is a sector photometer. There are two openings, one is now small and the other large. If I turn one disc upon the other so as to get a full opening of one sector I shall entirely close the other. When it is rotated you have two rings of light. If we twist the disks we shorten the length of one ring, and make a corresponding opening on the other. We have two lights passing through, one through the small opening and the other through the large. The large is the standard candle, and the small one the arc light. These can be thrown at a distance of a metre from the screen; both lights are at the same distance, and therefore the angular value is the same; it gives a direct measure of the intensity of the two lights. Then comes the question of the hetero-chromatic trouble. In this disk when it is fully opened there is 50 per cent. of obscuration. This light shows a penumbra, and the penumbra increases in proportion to the luminosity. If we take an arc light—that is, 2,000 candles—then a slit would be opened $\frac{1}{2000}$, and this slit would represent 1,999 divisions. Therefore we get on the scale this indication. The divisions are numbered, and read from right to left and left to right. We then have the index pointing to 1,999 on the one reading, and one division on the other. It is a direct-reading photometer, and can be used in a very small space. As to the hetero-chromatic effect, having a minimum of 50 per cent. penumbra, any slight variation in colour, pink or green light, for instance, that slight amount of tint which is very sensitive to the eye and so fallacious when seen on a Bunsen screen, is entirely obliterated by the penumbra obscuration of the disk. I can compare it in this way. Supposing we take two tumblers of water, one with a little green paint in it and the other a little pink paint. The contrast between those two is very great. Put a little Indian ink into both the tumblers and you cannot tell which is the pink tumbler and which is the green tumbler. So this penumbra of the disk gets over the difficulty of the hetero-chromatic effect.

Mr. L. GASTER: I think that Professor Fleming is to be congratulated on bringing such a valuable paper before us. Regarding the 10-candle-power pentane standard, I should be very much obliged if Mr. Vernon Harcourt would give us some information as to the price of pentane and the price of the lamp, because that might have an influence upon some of the people who wish to use the lamp for their photometric work. The amyl-acetate lamp is very cheap whereas this one Mr. Gaster.

Gaster. is rather more expensive. From what I have been given to understand, when once the lamp is adjusted it works very well, but I should like to know whether there are any difficulties in manufacturing the lamps so as to make them to agree sufficiently with one another to serve as reliable standards. In order to avoid any doubts on the accuracy of each standard I would venture to suggest that every lamp sold should be first tested at the National Physical Laboratory, and each lamp should be accompanied with a certificate of approval, after having been compared with a recognised standard of light.

Professor Fleming deserves our thanks for his important researches on the use of the incandescent lamp as a standard. This form of standard promises to be largely used because it is much easier to keep the lamp under the prescribed conditions so as to yield the standard unit of light, if the lamp has once been tested against an admitted standard.

Professor Fleming has suggested that every lampmaker should supply with his manufactured lamps the polar curve of their luminous intensity; but I must point out that not all manufacturers supply also the carbons for their lamps, and as improvements are constantly being made in the manufacture of carbons, several patents have only recently been taken out in this direction. I think that it will be necessary first to settle upon a standard carbon with which the tests shall be carried out, giving the dimensions of the carbons and the current and voltages used, as all these conditions contribute to affect the results obtained.

Regarding the question of photometers, I see that Professor Fleming mentions also Blondel's photometer, and Mr. Blondel's work in this direction is well appreciated, but I wish to add that if the Professor would have continued his valuable researches a little further than 1896 he would have come upon a very interesting improvement on Blondel's photometer, described by Prof. C. P. Matthews at the meeting of the American Institute of Electrical Engineers on September 27, 1901. [See *Science Abstracts,* No. 623 of 1902, and 1478 of 1902.] With the aid of this photometer you can practically get in one reading an illumination upon the photometer screen proportional to the mean spherical intensity of the source, which may be an open or enclosed arc lamp, simplifying the process of photometry very considerably.

With regard to the units suggested by the author I quite agree with nearly all the remarks he has made. In using as basis 10 and the metre, he brings us nearer to the Continental countries, and I hope before long that the metric system will be used extensively also in this country. Considering the difficulties to which the Professor alluded in the selection of an international standard of light, it is a great pity that the question of nationality should be involved; if science is to be worthy of the name it should not recognise any such boundaries, and if a good standard has been worked out in one country and is the best existing, it ought to be adopted all over the world and no national feelings should interfere with its adoption. The 10-candle-power pentane lamp of Mr. Harcourt, the incandescent lamp described by the Professor, and the achievements with the platinum standard ought to

be carefully studied by those interested in the matter so that at the next International Congress of Electrical Engineers there should be ample material brought before the meeting so as to come to a better understanding regarding the adoption of one international standard of light, to the benefit of all parties concerned—professors, manufacturers, and consumers. From my personal experience in testing incandescent lamps, I can corroborate Professor Fleming's remarks regarding the great discrepancy prevailing in marking the lamps to-day. I most heartily thank Professor Fleming for his work as one interested in photometry work generally, and particularly in the manufacture of carbons. Mr. Gaster.

Mr. H. E. MOUL: Any remarks that I may make are intended to apply only to the commercial and not the scientific side of incandescent lamp photometry. I am not open to make any predictions with regard to the scientific side, but it seems to me that as engineers we have a great interest in it, and I think, here at all events in England, we do very little with it. Dr. Fleming opens his paper by pointing out the differences in tests by different observers, at different times, and I suggest that a standard for instruments is almost as essential as a standard of light. Practically it comes to this: no photometric results are really comparable when taken by different observers with different instruments. These accuracies of ½ per cent. and 1 per cent. do not then occur in practice. It seems to me that if comparative tests are to be made at different places, one in Germany and one in England and so on, one must adopt a standard class of photometer for this purpose, just as much as a standard source of light. Experience shows that it is only by using similar instruments that different tests have been made to come within ½ per cent. of each other. As regards the actual photometer head, any one who has worked with a Lummer-Brodhun will never go back to the old grease-spot photometer, but the Lummer-Brodhun instrument is now being superseded by the Krüss type with straight telescope and contrast field. As engineers, we are not interested so much in what is to be the actual light standard as in getting a standard at all; we cannot in actual commercial work refer any readings we take to a standard over here. There is none, and the net result of this is that lamp-makers supply what they think is a good thing, and the central-station engineers say it is a bad thing, and really neither party knows what they are talking about. If they think they know it they have no standard for reference and no place from which to obtain any legal decision on the question. While this battle of the standards is going on we ought to have one class of standard to which readings can be correlated, even if something else should be adopted afterwards. Unless every central-station engineer is going to set up his own standard, he wants some calibrated instrument other than what we have seen here; standardised lamps are the right thing for this purpose. On the Continent you can get them in big bulbs and certified by the Reichsanstalt at a cost of half-a-crown each, with the position marked in which they have been standardised, and you know that they are sufficiently accurate for commercial purposes. Mr. Moul.

The use of potentiometers is very pretty in theory in the laboratory,

Moul. but how they will turn out in the ordinary course of everyday work I do not know. When stations supply lamps, which will in the future be the case, and 5,000 lamps are daily put through the photometer-room, what will be the state of the potentiometer then? In practical work, to put that quantity through a photometer-room, D'Arsonval reflecting galvanometers answer every requirement.

Dr. Fleming has shown a diagram of this balancing means of photometry. It is not cited in the references, but practically that is the Strecker type of photometer which was illustrated and described in Germany in 1888 (*Strecker Hülfsbuch*, 1888, p. 267), and is now adopted by the German Institution of Electrical Engineers, only in this case the mirrors have been left out. Behind the lamp under test there are two mirrors, five inches square, at an angle of 120°, and the apex of these is placed at the zero of the bar.

Dr. Fleming has made one statement with regard to lamp manufacture which I venture to contradict. He says that lamps blacken less the worse the vacuum. That may be, but it does not mean that if you get a lamp that does not blacken, the vacuum is bad. Possibly this was the case with the antiquated methods of pumping which are gradually being eliminated here, but in standard practice it does not apply. I have laid on the table the report and the lamps of a life-test at 2·5 watts per candle (Harcourt Pentane Unit) by the Reichsanstalt in Berlin in which blackening was not noticeable in the lamps tested. I have done this to show also the conveniences that a manufacturer has over there in regard to this class of work. There are seven lamps on the table which have been tested at the Reichsanstalt, and each lamp is marked with the Government mark, and there is the legal certificate in connection with it. If there is any dispute here, one side calls in an expert, and the other side, if it can afford it, calls in three, and they each fight it out, and we are none the better for it, but are left with a big bill to pay. We have a Reichsanstalt here, and we are still a long way off a standard; in the meantime we are being used as a dumping-ground for every bad lamp manufactured abroad simply because the foreigner knows that we have not a standard, whereas they have, and so cannot sell such lamps at home. It is time we did have a standard; the Reichsanstalt here ought to give us just as much as we can get over there; we want a standard and lamps calibrated in terms of the standard selected. If we could get these lamps they would give us a means of checking our supplies, and if then there is dispute between maker and customer, we have our Reichsanstalt to refer to; equally it ought to be possible for our Anstalt to authorise some of our other laboratories such as Owens College, University College, and others to undertake work of this description and give certificates equally valid with those of the Physical Laboratory. This power to test to Imperial standards and issue legal certificates concerning the tests has been delegated with the happiest results to several of the leading University laboratories in Germany by their Reichsanstalt.

Prof. W. E. Ayrton: We must congratulate Dr. Fleming on having *given us a most* interesting paper on a most interesting subject. He

Prof. Ayrton.

commences by pointing out what is most true—what the President pointed out in his inaugural address—viz., that while central station engineers devote a great deal of attention to the efficiency of their boilers, engines, and dynamos, and are aghast at an extra loss of 1 per cent. in the generating plant, they seem to be, if I may say so, a little oblivious of the fact that in this city, at any rate at the present time, the main use of the electric current is to supply light, and therefore the glow-lamp is just as important as the boiler, or the dynamo or the steam engine. American electric light companies so thoroughly appreciate this that they, as you probably all know, insist on supplying lamps, and would almost prefer to *give* lamps than to leave the consumer to buy inferior ones. If you want to see what has been done in America in the testing of lamps, I would refer you to an almost extraordinary report of tests that has been carried out by the Standard Oil Company of America, who wanted to find out, not only in a technical way, in a scientific way, but a commercial way—which is scientific, of course—what qualities of lamps could be obtained in the United States. I will not go into the report which has been issued by the General Electric Company of America under the title "Agents' Handbook of Lamp Tests," but I may tell you that it was most interesting to me, and I am sure it will be to you when you read it, to find that a commercial company like the Standard Oil Company had carried out such an excellent piece of work.

Dr. Fleming draws attention to the vagueness in the testing of lamps. I have referred to that on previous occasions in public, and when I have done so it has, I fear, been considered that I was exaggerating. He speaks about a difference of 25 per cent. I will give you an instance, because it happens to deal specially with photometry, that occurred about a year ago, where the difference in the results of the light tests was more like 33 per cent.

A certain firm in this country which makes lamps sent me some 16-candle power lamps to test. These were not lamps bought in the open market; they were not lamps chosen by me at random from the maker's stock, but they were presumably selected lamps, because they were sent to me by the makers. My report of such lamps, no doubt, might have been too favourable to the makers, but you could hardly have expected the reverse. Yet when I tested the lamps I found that instead of getting 16 candles—they were life tests, of course—the life curve of some of these lamps never showed more than 10·8 candles during the 600 hours' run; the average candle-power was 7·85, with average inefficiency of 5·6 watts per candle. That was the class of lamp sent me a year ago by the makers themselves. Now this is what they wrote me when I sent them the report: "We express our disappointment at the results. The curious facts about these tests is, that two other tests were made at the same time upon the same batch of lamps, independent of your own tests here on the two mentioned, and each gives the candle-power of the lamps at 16 candles." So that, as there were three tests against me, it seemed rather a staggerer. Of course I examined the measuring instruments most carefully—the ammeter, the voltmeter, and particularly, of course, the standard of

Prof. Ayrton.

light. I was using then, and still use, among other standards, the Dibdin 10-candle standard—a standard not referred to by Dr. Fleming in his paper, but which was reported on so highly by the Committee of the Board of Trade in 1895. That Committee went so far as to say: "We therefore recommend that a pentane-air flame, furnished with a Dibdin-argand burner, having the form and dimensions set forth in the Appendix (Section IX.) and used in the manner there defined, be accepted as giving the light of 10 standard candles, and that this flame be authorised and prescribed for official use in testing the illuminating power of the gas supplied by the London Gas Companies." That was the recommendation in 1895, and I used such a standard. Mr. Dibdin was kind enough to test my specimen of this standard against the actual specimen which had been examined by the Board of Trade Committee, and still we could find no error in our tests of the glow-lamps. I therefore reported to this particular company that what I sent was my final result, and that in spite of the three independent tests giving results differing from my own, the lamps did not give 16 candles, but some of them never gave more than 10·8 candles at the specified pressure at any period of the test. I then asked them would they give me the names of the persons, whom they thought most trustworthy, who had tested the lamps, and I would endeavour to run the matter home and find out the real cause of the discrepancy. They did so, and I found certain errors in the ammeters and voltmeters. I will not go into that question now, because it is purely an electrical question, although it was necessarily concerned with the photometry of the lamps. I will deal now with the photometric part alone, especially as it concerns a point not referred to by Dr. Fleming.

The standard of light employed by these persons was the Harcourt pentane lamp, but not, however, the more recent pentane standard. The present forms of the 1-candle and of the 10-candle standards have no wick, but in the old days the 1-candle standard Harcourt pentane lamp had a wick, and I found that a great deal depended on the way in which the wick was treated. Messrs. Woodhouse & Rawson, the makers, when they sent out this pentane lamp in 1890, issued certain printed directions regarding the mode of using the Harcourt pentane lamp, but the directions were not sufficient, because I found that by not paying proper attention to certain precautions not clearly mentioned in these directions you could get a difference in the light given by the lamp. The difference between my estimation of the light given by the glow-lamps and the result given by the other experimenters was about 33 per cent., *i.e.*, 10·8 candles as compared with 16 candles, which is about 33 per cent., and I found that a portion of this difference was due to the light given out by the Harcourt pentane standard lamp as used by the firm of testers in question being less than the light given out by the similar Harcourt pentane standard lamp in my laboratory. In the instructions one is told to light the lamp by inserting a piece of cotton-wool dipped into spirits between the outer cylinder and the burner. The piece of cotton-wool cannot easily be inserted from below even if one were told to do so, but as one is not, one naturally tries to light the

lamp by inserting it from above, which it is easy to do. But the lamp will not light this way for some time, so that any ordinary person who has perhaps used paraffin oil lamps all his life naturally turns up the wick; still the lamp does not light, so he turns the wick up more until the wick protrudes from the burner, until perhaps the wick is turned up so much that the top becomes visible when the lamp is looked at horizontally. But that is all wrong, if the poor user only knew it, for then the top of the wick is liable to char. Prof. Ayrton.

Now what the user ought to be told is this: First insert the piece of lighted cotton-wool between the outer tube and the burner FROM BELOW; do *not* turn up the wick even if the lamp does not light, but leave the wick exactly as it was when the lamp was last used, and *always* not only start the ignition but cause the flame to mount until its tip is visible in the slot by simply warming the outer tube with a spirit lamp. Prof. Vernon Harcourt has been so kind as to give my assistants and myself a personal demonstration in my laboratory of how to use this pentane lamp, but for the benefit of those who have not had this advantage I mention all this to avoid their making the mistakes into which this firm of testers unintentionally fell when they brought out that the light of the glow-lamps were 16-candle ones, while we proved that some of them never gave more than 10·8 candles at any time at the specified voltage.

But there are other sources of error in the instructions issued with these wick pentane standard lamps, for one is told therein that the flame should be adjusted so that the tip is visible between the upper and lower edges of the slot; whereas, as far back as 1895, my students pointed out to me as the result of their tests that the Harcourt pentane standard lamp gave nearly 4 per cent. more light when the wick was turned up so that the tip of the flame was at the middle of the slot than it did when the tip of the flame appeared just above the bottom of the slot.

Lastly, there are differences between the amount of light emitted by different specimens of these lamps even although the lamps generally appear to be of the same form, and pentane, obtained from Messrs. Miller, of Oxford, is taken from the same can to fill the glass reservoirs of the two lamps up to the same level. Mr. Robertson, of glow-lamp reputation, has kindly lent me his specimen of the Harcourt pentane standard lamp, and many comparisons have been made between his lamp and our own by Messrs. McEwen and Dow, two of the assistants at the Central Technical College, with the following results—the lamps being called respectively the "Robertson" and the "C.T.C.," although, of course, neither Mr. Robertson nor the Central Technical College are responsible for the construction of these two lamps. It should be mentioned for those who are not familiar with this type of lamp, that two cylindrical metal blocks are supplied, with each lamp, marked respectively 1 and 1·5, and it is supposed that when the aperture for the emission of the light is carefully adjusted with one or other of these blocks or distance pieces, and the wick lighted in the proper way and the other precautions adopted, that the lamp becomes a one or a one-and-a-half *candle standard respectively*.

Prof. Ayrton.

Well, these were the results experimentally obtained :—

I. $\frac{\text{Robertson 1 candle}}{\text{C.T.C. 1 candle}} = 1{\cdot}05$ even when the *same* block (viz., the *Robertson* 1) is used to adjust *both* lamps and the flame is adjusted to the *centre* of the slot in each case.

II. $\frac{\text{Robertson } 1\frac{1}{2} \text{ candle}}{\text{Robertson 1 candle}} = 1{\cdot}40$ when the *Robertson* 1½ and 1 blocks are successively used in the *Robertson lamp itself* and the candle-power compared with another standard of fixed, but not necessarily known, value. Flame in Robertson lamp kept adjusted to *centre* in *both* comparisons.

III. $\frac{\text{C.T.C. } 1\frac{1}{2} \text{ candle}}{\text{C.T.C. 1 candle}} = 1{\cdot}45$ when the *Robertson* 1½ and 1 blocks are successively used in the C.T.C. *lamp itself* and the candle-power compared with another standard of light of fixed, but not necessarily known, value. Flame in C.T.C. lamp kept adjusted to centre in *both* comparisons.

IV. $\frac{\text{C.T.C. } 1\frac{1}{2} \text{ candle}}{\text{C.T.C. 1 candle}} = 1{\cdot}51$ when the above experiment, No. III., is carried out with the *C.T.C. lamp*, but when the *C.T.C.* 1½ and 1 blocks are used instead of the two *Robertson* blocks, etc.

V. $\frac{\text{Robertson 1 candle, Robertson block used}}{\text{Robertson 1 candle, C.T.C. block used}} = 1{\cdot}035$

Hence, combining the above results, it follows that if you test the light of, say, a glow-lamp, using the Robertson pentane lamp with the Robertson 1 candle-power block, and with the tip of the flame at the centre of the slot, next test the same light with the C.T.C. pentane lamp, using the C.T.C. 1 candle-power block but with the tip of the flame at the bottom of the slot, you will obtain about $5 + 3{\cdot}5 + 4$, or about 12·5 per cent. difference in the estimation of the light of the glow-lamp.

Some tests subsequently made through the kindness of Prof. Vernon Harcourt and under his superintendence, at the testing-room of the Gas Referees in Victoria Street, show that if these two lamps respectively be used, each with its own 1 candle-power block, as supplied by the makers, and the tips of the flames be adjusted as described above, the estimation of the light of a glow-lamp, say, will differ by the amount already mentioned, that is about 12·5 per cent., although the second set of tests divides this error somewhat differently among the different causes.

In Professor Vernon Harcourt's patent specification No. 11,985, of *1887, the* dimensions of each of seventeen parts of this type of lamp are

given. On having measurements made of these parts in three specimens of this lamp, which are at present in my laboratory, I find that all three lamps differ from one another, and that no one of the three has exactly the dimensions given in the patent specification. Prof. Ayrton.

Of course these variations are due *not* to faulty design on the part of the inventor, but to errors in mechanical construction of these pentane standard lamps and to imperfections in the printed instructions for using them which have been sent out by the makers.

Further there is one other difficulty we always have to deal with, which has been raised by the last speaker. What do we mean by a candle? Some people insist on saying that they mean by a candle a candle which is made in Germany, as most things are now made in Germany. Dr. Fleming shows you what great differences have been found in the comparisons between the amyl acetate standards and the British standards, and he puts down those differences to the method of testing. I go further than that. I venture to think that the amyl acetate standards that have come over to this country, and which have come into the laboratories, differ a good deal one from the other. So it is not merely a difference in the method of testing, but the candle made in Germany is not always the same candle. Mr. Moul said that anybody who had used the Lummer-Brodhun photometer would never go back to anything else. I was talking about that only this week to the authority on photometry who spoke on the previous occasion, Sir William Abney. He said his opinion was quite the contrary. He frequently has men come into his laboratory to work who come with a predilection in favour of the Lummer-Brodhun photometer, but they almost invariably before they leave him go back to the old Rumford photometer. What is the enormous value of the Rumford photometer? It is that it is not made erroneous by stray light. A great many errors are introduced into photometric work by stray light, and all photometers which have light coming in on both sides are susceptible to this error. Sir William Abney says that he considers the Rumford photometer far superior to the Lummer-Brodhun, or any photometer in which the light comes in on the two sides. I have had brought here for a variety of reasons a photometer which a colleague and myself made nearly twenty-one years ago. We were put in a great difficulty at the Paris Exhibition of 1881. We had to report on the different types of lamps in the Exhibition. The report was published in *The Engineer* newspaper afterwards. We had to devise a portable photometer for the purpose, because the lamps were in all parts of the exhibition. *This* photometer was, therefore, devised and called the "Dispersion Photometer."

(The photometer was exhibited.)

It has one thing which I may draw attention to, because from what Dr. Fleming has said the importance of it does not seem to be appreciated even now, namely the 45° mirror. He pointed out that people still—and it shows how beautifully conservative our country is—make photometers with a mirror turning about an axis in the plane of the mirror like an ordinary toilet-table looking-glass. The consequence is,

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that the amount of light absorbed varies with the way in which the light falls, and is reflected. Instead of turning your mirror in that fashion you ought to turn it about an axis making an angle of 45° with its plane, for then the light always falls at the same angle on the mirror, and always leaves at the same angle, and the proportion absorbed is constant. The use of this 45° mirror Dr. Fleming draws attention to in connection with his arc-lamp arrangement, on page 146 of his paper. I can testify that that arrangement is an extremely good one for getting the polar curve of the arc-lamp. The method, as he has already explained, is to compare the light given off by the arc in any direction with the light given off horizontally by the same source. I happen to know a good deal about the apparatus, because it was devised and constructed by an assistant I had several years ago, a Mr. Carter, who, like many other of my men, has been absorbed by the General Electric Company of America. Mr. Carter later on published a series of articles in the *Electrical Review* on Photometry, and in the article of August 3, 1900, he gives a complete account of the way of using this 45° mirror to accomplish the result that Dr. Fleming has achieved on p. 146 of his paper. He has, in fact, practically arrived at the same arrangement, so that you can compare the light going out from the arc at any angle with the light sent out at a particular moment horizontally. Mr. Carter's work is also the more interesting because in this series of articles—there are five or six of them published in the *Electrical Review* in the middle of 1900—he used the "distinctiveness" photometer, I think he called it. *This* is one of the paraffin blocks he employed. I daresay you all know the Joly paraffin block photometer, where you have two blocks of paraffin wax separated by tinfoil. There is nothing new in that, but what was new in this photometer was the ruling of the photometer screen used by Mr. Carter, the arrangement of lines getting finer and finer, only in this case he used radial lines. But Mr. Carter, in spite of his doing most excellent work on photometry at the Central Technical College, as published in the *Electrical Review*, I think made the same mistake that Dr. Fleming has made. In using the "distinctiveness of vision photometer," you are not measuring the brightness of light at all. Sir William Abney said a word or two about this on the last occasion, and I want to emphasise the criticism Sir William offered. There are many things that are interesting in connection with two lights—how much it cost to produce them, what you pay to buy the lamps, what is their relative weight, etc.—but there is one thing which is particularly interesting, their relative brightness, and that is what the photometer has to measure. I do not think that what have been called distinctness of vision photometers are photometers at all, and I do not think they have anything to do with photometry. A photometer is used for the purpose of comparing the brightness of two lights, and not for finding out how easily you can see something or other in either of the lights, for that depends on something totally different from the brightness of the lights. Therefore I think that Mr. Carter, when he devised that arrangement described here, fell into the same mistake that Dr. Fleming has fallen into.

Prof. Ayrton.

Dr. Fleming takes the view that you cannot compare two different coloured lights. There again, if he will allow me to say so, I think he has fallen into a very big error. He laughs at the idea of using coloured glass. You remember he made some humorous remarks to you on the last occasion, and obtained the laugh of the audience, but that little joke was directed at me, and as he has thrown down the gauntlet I am quite willing to take it up. In this photometer which Prof. Perry and I used at the Paris Electrical Exhibition of 1881, we had attached to it a frame containing red and green glass. Dr. Fleming says that the old method of looking at the photometer screen through red and green glass is quite unscientific. How is it unscientific? He would not say, I suppose, that spectro-photometry was unscientific. Spectro-photometry has a long name attached to it, and as I know to my cost, you have to buy a rather expensive instrument if you want to do it well. But what does spectro-photometry do? It simply takes a bit of the spectra given by two different lights, and compares the brightness of those two portions of the two spectra. What does this frame with the red and green glass in it do but that? Suppose you take a piece of what is technically called ruby-red glass and a piece of signal green glass, and supposing you take the precaution, as we used to, of selecting our red and green glass, so that on putting them one over the other you almost have blackness when a bright light is looked at, then you first make your photometer balance with the red glass and then with the green glass, are you not really performing spectro-photometry? So far from thinking it unscientific, I think it is most useful. But you may say, "How can two lights have a totally different ratio, being, say, 3 to 1 and also 2 to 1?" Quite easily —why not? If you want to look at the red roses, then the arc bears to the candle a certain proportion: if you want to look at the green leaves attached to the red roses, then it bears a certain totally different proportion, and that is what you mean. In looking at the red flowers the arc is so many times as good as the candle: in looking at the green leaves the goodness is something totally different. I go a step further. Dr. Fleming referred us last time in his interesting and most instructive paper—I must congratulate him on the way in which he delivered it, I envy his power of putting it before you—he referred to Purkinje's experiment. He had a Ritchie wedge, illuminated by a glow-lamp behind a piece of red glass, and another glow-lamp with a piece of green glass before it, and he told you, and possibly showed you after the reading of his paper, that if you put the wedge so that the two lights balanced, that is the brightness appeared to be the same, and then if you doubled the distance of each lamp from the wedge, you would no longer get the balance. It is simply an optical delusion with weak lights, and the phenomenon does not exist at all with ordinary lights. If it were true it would mean that colour photography was a snare. Is it or is it not a fact that if by proper means you balance a red light against a green light, and then double the distance of each light from the photometric screen, you will get a balance the second time? I can assure you you can. There are two ways in which you can compare them. You can compare the red and

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the green light looking through green glass: and then if you alter the distance you will find the inverse square law holds. If you compare them with the red glass, you will find again that the inverse square holds. That is, if one is three times the other at one distance, it will be three times the other at any other distance. Let me go further; leave the glass on one side. It is quite possible to compare a red light with a blue light without using any coloured glass at all, and to get marvellous accuracy. The secret was given by Sir William Abney, but it is so absurdly simple that I want again to impress it upon you, because the result is wonderful. Mr. Medley, in a paper which he read, with myself, before the Physical Society in 1895, described how, by taking two different coloured lights, you could get the same measurements over and over again, within $\frac{1}{3}$ per cent., without any coloured glass at all. The secret is this. First, you oscillate the photometer until you get the best balance you can, then you oscillate one of the standards, one person oscillating it while the second person is getting a final adjustment of the photometer. I will give you the actual results we have obtained, and they will show you the sort of accuracy with which it can be done. We compared a green light with a yellow light. The yellow light was a 100-volt glow-lamp underrun at 83 volts: the green light a 100-volt glow-lamp overrun at 107 volts, green gelatine being in front of the latter. They were first put at 140 centimetres distance, and the measured ratio of the one to the other, when properly tested, was 1·51. They were then put at double the distance, 280 centimetres, and the measured ratio of the one to the other was 1·52, almost exactly the same, in spite of Purkinje's experiment. Next we compared the underrun lamp with the overrun lamp when red instead of green gelatine was put in front of the latter, that is we compared a dullish yellow with a bright red light, and the result obtained at the 140 centimetres distance between the lamps agreed even still more closely than before with that obtained when they were separated by the 280 centimetres.

Hence it is certain that whatever may be found with very *dull* lights—if two totally differently coloured glow-lamps, even when one is underrun and the other overrun, are *properly* compared, the same result will be obtained whether they are at 140 centimetres distance or at 280 centimetres.

Mr. Patchell.

Mr. W. H. PATCHELL: I was brought into this matter as when considering raising our pressure to 200 volts, I was constantly met with the assertion that the 200-volt lamp was not so good as the 100. I looked up the authorities, particularly those mentioned at the Board of Trade inquiry about a couple of years ago, and I found, like the two Professors on either side of me, that they practically cancelled out,—you could believe whichever you liked. I therefore thought the only thing to do was to go into the subject myself, and looked round for a photometer. I only wish I had had Dr. Fleming's paper by me at the time, because it would have saved me much trouble, and I feel we are greatly indebted to him for the concise way in which he has placed the subject before us. *Mr. Edgcumbe* said the paper was a little disheartening. So it is. But a great many other things are. I constantly get circulars saying that if I

use somebody's engine I can save 25 per cent. of my steam: by using sombody else's oil another 10 per cent., by using somebody else's waste and somebody else's packings I can save 30 per cent., so that if I used everything as per advertisement I might be actually selling coal instead of buying it! Still we do not get disheartened. I feel we must be thankful for what we can get, and go on with the photometers that the Professors have provided us with. After looking round I settled on the photometer which Dr. Fleming mentions on p. 123 of his paper, made by Wright of Westminster, with the double candle standard. It is the Pentane standard, which I certainly prefer to the Hefner standard, but I had leanings towards the other disk, the Lummer-Brodhun. When I found I could not get a sort of cross-bred photometer, I took Wright's as I found it, with the exception that I insisted on having a scale calibrated in candle-power, and you would be surprised at the powers of persuasion that were necessary to get that carried through. At last we succeeded, and then we began our measurements. We were not in the happy position apparently, by the results given, that Dr. Fleming is in, nor did I say that I wanted lamps for testing, and as I was not a Professor they were not picked and sent to me. I had them sent in batches of half a dozen, 8 and 16 c.p.; 100 and 200 volts. On p. 163 Dr. Fleming refers to 8-candle lamps which were as a matter of fact nearer to 10-candle at 30 watts. I wish I could get them, because they were absent from the samples I had sent in. We found that in samples tested by us the 16-candle 100-volt lamp had an average candle-power of 12·76, and an average inefficiency of 4·8 watts, while for 200 volts the candle-power was 12·80 and the watts 4·72. The 8-candle 100-volt had an average candle-power of 7·76, and an efficiency of 4·24 watts, while the 200-volt lamps gave 8 c.p. for an average 4·20 watts. Therefore we did not get the 30-watt lamp at 10 candles. If the reading of this paper will stimulate the lamp manufacturers to send us better lamps, we shall be thankful to them. Mr. Patchell.

Mr. ALBERT CAMPBELL: The amyl-acetate lamp has been rather disparaged by Dr. Fleming, but I think it is not so bad as he makes it. For example, the colour of its light does not compare very unfavourably with that of a glow-lamp that has run more than half its normal life, and it is important to test glow-lamps in such condition. The work done by the German Reichsanstalt, using the Hefner lamp as standard, seems to me to afford the best proof of its practicability. I have compared, with the greatest care, glow-lamps tested seven or eight years ago at the Reichsanstalt with others tested there quite recently, and in all cases I have found very exact agreement between the actual relative brightness and the numbers given in the certificates, the agreement being well within 1 per cent. This seems fairly conclusive evidence that the amyl-acetate lamp can be worked as a trustworthy standard. Mr. Campbell.

In using some flame standards it is important to guard against vibration. For instance, in Simmance's wickless pentane lamp, in which the flame is not screened in any way, I have noticed that very slight vibration has a large effect on the height of the flame and upon the candle-power.

Mr. Campbell. With regard to Dr. Fleming's proposed new unit of candle-power, for two reasons I think the change undesirable. First, from the linguistic point of view it is wrong to take as the name of a new unit any common word which has already a clear and definite meaning. In the second place, a unit should be of such a size that the quantities in common use can be expressed with fair accuracy without fractions. Thus, to talk of "5, 16, or 55 candle-power" seems much more convenient than "0·5, 1·6, or 5·5 lamps." The fact that for ordinary glow-lamp testing it is best to use a *standard* of 10 or 20 candle-power is no reason for making the *unit* of similar magnitude.

Mr. Lacey. Mr. T. S. LACEY: As one who is engaged in the gas industry I should like to make a few remarks. I am familiar with the one-candle standard of Prof. Vernon Harcourt, and also the 10-candle standard. I think it is hardly correct to give Mr. Harcourt's name to standards that are not arranged and devised by him. The 1-candle standard is well known, and is very accurately described, and also the 10-candle standard, but it is quite possible that other modifications may not have the same light value. In relation to the question of flame standards, they give a light varying according to the conditions of the atmosphere, barometric pressure, and so forth, but gas engineers are not concerned with this variation, provided that the standard varies in the same proportion as the light which they have to supply and with which it is compared, but it does not; and that involves certain difficulties. Small flame units (especially those of a feeble and flickering character, like the 1-candle, and, I should imagine, the Hefner standard) will be found to be more subject to atmospheric contamination and differences of water vapour than the larger ones. The 10-candle standard is certainly less affected by carbonic acid and water vapour than are sperm candles, and the Argand burner is less affected than the Harcourt standard. Until we have some determinations of the behaviour of these standards, under different conditions of atmosphere, it would be unwise to pledge ourselves to the use of small standards. The Dibdin pentane standard is a convenient one to use, but I think attention has been drawn to the difficulty of the chimney; if you break it, you break your standard.

M. J. Violle. M. J. VIOLLE (*communicated and translated*): I desire in the first place to convey to Dr. Fleming my cordial acknowledgment of his courtesy and impartiality towards myself. Mr. Petavel's very careful study of my standard has already removed the majority of the objections to it that have been raised after superficial examination. A searching investigation of the kind that Dr. Fleming would like the National Physical Laboratory to undertake would show that, even in the direction suggested by Mr. Vernon Harcourt, there is no serious obstacle to its complete realisation. I hesitate, however, to add anything to that which Dr. Fleming has been good enough to say concerning my standard.

I will not criticise Mr. Vernon Harcourt's lamp, since I have only had an opportunity of experimenting with an imperfect specimen. His practical 10-candle standard, as I understand it from Dr. Fleming's account, appears to me, however, to be exceedingly well thought out.

In regard to flame standards, I have not only experimented with a hexane standard similar in principle to that using pentane, but I have also devoted attention to the production of an acetylene standard,[1] which, although not yet perfected as completely as I could wish, appears to possess a sufficient degree of accuracy, whilst at the same time having that degree of simplicity and convenience in use which we rightly expect to find in a practical standard. M. J. Violle.

With reference to the practical standard proposed by Dr. Fleming, I feel convinced that the incandescent lamp should be completely satisfactory when employed in the form and under the conditions prescribed by the author. The large-bulbed Fleming-Ediswan lamp will henceforth have a conspicuous place in the photometric laboratory whether of the electrical engineer or of the gas manufacturer.

Mr. J. E. PETAVEL (*communicated*): Within the limited space which can be disposed of for the purposes of discussion it is not possible to go in detail into the many interesting problems raised by Dr. Fleming's most valuable paper. A few words, however, on the general aspects of the question may be of some interest. Mr. Petavel.

Were the matter not so generally overlooked, it would be trivial to emphasize the commercial importance of the subject, but strange to say whereas the greater part of the huge capital involved in the electrical industry in this country is actually devoted to the production of light, the consumer remains content to measure the amount of energy delivered, leaving the question of the quantity of light more or less to chance. The general callousness with regard to this subject is not more favourable to the best manufacturers than to the public at large. It is a somewhat strange anomaly that the same merchant who would get into serious trouble for selling as a pound of tea a packet containing 15 ounces, may without let or hindrance import and sell as 16 candle power, lamps which will only give 13. In most cases the purchaser will make no verification whatever, or perhaps, measuring only the current consumed, will express his delight at the high efficiency of the lamps. This discrepancy, even if detected, is of too usual occurrence to call for any special notice or condemnation.

I do not wish in any way to minimise the inherent inaccuracies which are necessarily connected with photometric work, or to detract from the importance of the right understanding of the difference between "luminous" and "visual" intensity. So long, however, as differences of 25 per cent. can exist between the values as determined by two different "testing laboratories,"[2] the above questions cannot be considered of immediate practical importance.

As far as the interest of the general public is concerned, the value of the work which is now carried out under the direction of the Metropolitan Gas Referees can hardly be overestimated. Unfortunately it is, *per se*, limited in its scope, and before any real reform can be hoped for the entire question of the measurement of artificial illumination must be taken up in a similar manner. As a plea in favour of the *laisser*

[1] This research has already extended over several years (see *Comptes rendus*, 1896, vol. 122, p. 179).

[2] See Dr. *Fleming's paper, page 119.*

Mr. Petavel. *faire* policy, it has repeatedly been urged that exact photometric measurements are difficult or impossible, but any one conversant with the subject will readily admit that, with many of the photometers and standards at present in use, an accuracy of two per cent. is easily obtained. Higher accuracy can undoubtedly be reached, and is most desirable, but measurements even if only to two or three per cent., if generally carried out, would be a vast improvement on the present state of affairs.

Turning now from the question of the actual measurements carried out (or not carried out) in everyday work, to the question of the legal standard. It is generally admitted that the British legal standard (the sperm candle) is, of all the standards in use, the most inaccurate and unsatisfactory; and yet, strange to say, no serious effort has been made to obtain a reform. It has been urged that a change should not be made until sufficient information has been obtained to warrant a definitive choice. The plea is undoubtedly well founded, but as the preliminary work must necessarily occupy some years, it would be most regrettable if time were wasted before making a start. The final choice will rest between the Violle molten Platinum standard, the Harcourt Pentane standard, and some standard based on the spectrum analysis of the radiation itself and resembling that proposed by Lummer and Kurlbaum. The work with regard both to time and expense is beyond the scope of individual research, and could be conducted successfully only at such an institution as the National Physical Laboratory, and it is satisfactory to hear from Dr. Glazebrook that steps are being taken in this direction.

From the preliminary experiments which have been made, the Violle standard seems to promise the best results. It has been proclaimed as the absolute standard of light by every International Electrical Congress, but has never been set up in such a manner as to be of practical use. The work carried out some years ago at the Davy-Faraday Laboratory has been kindly referred to by both Dr. Fleming and Dr. Glazebrook. A few further words on the subject may, however, be of interest. The electric method of fusing the platinum was not abandoned through any inherent difficulties, but simply because the preliminary results then required could be obtained in a less expensive manner, by means of an oxyhydrogen blow-pipe. The experiments were only rendered possible thanks to the kindness and generosity of the Directors of the Davy-Faraday Laboratory, but none the less in a private institution there are certain limitations which need not affect a research which is carried out by the National Physical Laboratory.

Thus the conditions under which this preliminary work was carried out were not of the most favourable character, but nevertheless, some well-defined results were obtained. These may be summarised as follows:—

1. That an increase or decrease of the total area of the molten platinum of as much as 40 per cent. will cause less than one per cent. variation in the light, *i.e.* the light is practically independent of the shape of the platinum ingot.

2. An alteration of 45 per cent. in the mass of the platinum ingot will cause a variation of intensity of the light of less than one per cent., *i.e.* the light is practically independent of the quantity of metal used. Mr. Petavel.

3. An alteration of 140 per cent. in the size of the aperture in the furnace cover will cause less than one per cent. variation in the light, *i.e.* the light is practically independent of the shape of the furnace.

4. The purity of the platinum is important, but the metal can be obtained commercially, quite sufficiently pure for the purpose.

5. The crucible should be of pure lime, which can, of course, be easily prepared in any quantity.

6. The hydrogen should be free from hydrocarbons. Coal gas is useless, but the hydrogen obtainable commercially was found satisfactory, the design of the blow-pipe being such that any small quantities of hydrocarbons would combine with the large excess of oxygen before reaching the metal surface.

7. The gases should be in the ratio of 4 volumes of hydrogen to 3 of oxygen, but as long as a considerable excess of oxygen is maintained the ratio may be varied within wide limits.

Personally I am not aware of any other case in which the specification of a standard can be varied within such wide limits without materially affecting the results. In the case of the Hefner lamp, for instance, one per cent. alteration in the height of the flame produces about one per cent. error in the light; it is therefore hardly necessary to increase the height of the flame by 40 per cent. to obtain a measureable variation.

As was pointed out in the original paper, the results though incomplete give some basis for the belief that if the Violle platinum standard is properly set up, and the experiments carried out according to uniform directions, its constancy will be greater than that of any other known source of light. Unfortunately it is not possible to push the experiments any further without considerable expense, and if the few hundred pounds necessary for the work are not available it will be far preferable to discard the Violle standard at once (as far as this country is concerned), and concentrate all available energy on the improvement of one of the less expensive standards.

Before closing, may I be allowed to join with the previous speakers in thanking Dr. Fleming for his paper, which for many years to come will be used as a work of reference by all those engaged in photometry.

The CHAIRMAN: I think we are very much indebted to Dr. Fleming for his paper. I should like myself, as it is an old subject of mine, to have talked on it nearly as much as Prof. Ayrton, but I will spare you that infliction, and call on Dr. Fleming to reply. The Chairman

Dr. FLEMING, in reply, said: At this late hour of the evening I shall not attempt to reply in detail to the many criticisms which have been passed on the paper. When I offered the paper to the Institution I felt sure it was a subject on which there would be a good deal of difference of opinion, even if not strong feeling, and that at any rate we should have an animated debate. That expectation has not been disappointed. In fact, it has been interesting to notice in the course of the debate how very much difference of opinion there has been, an Dr. Fleming.

Dr. Fleming,

how much that has been approved by some has been condemned by others. On the evening on which the paper was read I think the discussion dealt with purely scientific matters. I was anxious that the questions placed before you should not be merely questions of detail, and that the debate should not resolve itself merely into a discussion of the advantages of one or other form of photometer, or method of photometry, but that some of the broad and fundamental questions should be discussed. The points which are really important are not whether one person can compare a red and a green light within 0·5 per cent., whereas others cannot do it within 50 per cent., but they are whether our methods of photometry, or the things that we measure and define, are really the things that should be measured and defined. The real things that I was anxious should be discussed were questions of a more fundamental nature. For instance, in walking along the street you cannot but notice the immensely different effects that are produced by arc lamps, open or enclosed, flame arc lamps, gas lamps, and by every other kind of illuminant, and the question must force itself upon your attention, whether the so-called candle-power *is* a measure of the value of that particular illumination for the purpose of vision—in other words, of its power to enable us to see. Supposing you put the question to a non-technical person whether a particular room or street was well lit. What would he do? He would take out a newspaper and see if he could read, and if he could read he would say it was good. In that we are not concerned at all with slight differences of colour. The question of whether you can compare a red and green lamp within a fraction of 1 per cent. is immaterial when we regard it from the point of view of practical engineering. On the way home to-night, in the railway carriage, you will not be concerned with the recognition of the precise difference in tint or colour between a *Westminster Gazette* and a *Globe*, so far as the paper is concerned, but what you are concerned with is, whether you can discriminate the print. If you can you say the illumination is all right: if you cannot, you say something appropriate. That is the reason why I laid stress on a method of discrimination as a method of determining the relative values of the illuminating powers of two lights. On the other hand the question of the colour quality of a light is very important indeed for certain purposes. I had hoped that from so great an authority as Sir William Abney we might have heard something which would guide us in arriving if possible at a way of distinguishing the colour revealing property of illuminants so as to define precisely their suitability for illuminating, say, a picture gallery or a dye-house. This is something quite different from "candle-power." If a central-station engineer were to ask a shopkeeper what candle-power he requires outside his shop the shopkeeper would say he does not know anything about candle-power. What he wants is such a light as will render the goods in his window as visible as, or more visible than, they are by daylight, and if he secures that he secures all he requires, and I do not think our ordinary measures for candle-power do give a numerical value to an illumination of any kind which would enable us to decide that question. I must not take advantage of your indulgence to go now into the

different objections that have been raised. I will simply conclude by thanking you for the kind reception you have given to the paper. I was very much pleased at the ready welcome which was accorded to it by the President and the Council when I first offered it, as I felt sure that whatever might be the merits or demerits of the paper it dealt with a subject which should be discussed, and that we should have an interesting discussion on it, and I feel on the whole that that hope has been fulfilled. Dr. Fleming.

(*Communicated*): I am glad to have the opportunity of adding a few words more by way of reply to the discussion on my paper, as, owing to the lateness of the hour, the time afforded me at the end of the debate was rather short. In so doing I will not deal with the remarks of each speaker separately, but will group them under the several headings of the subject matter of the paper.

First as regards Standards of Lights. There seems to be a general consensus of opinion that the "candle" is now merely the name for an arbitrary unit of light, and that as far as the British candle is concerned, it is best represented either by the Hefner lamp with the flame height increased 14 per cent., or by the 1-candle Pentane lamp of Mr. Vernon Harcourt, or otherwise that 10-candle power is best represented by the new Vernon Harcourt 10-candle standard. On the other hand, the British standard "candle" considered merely as a unit, does not agree with those adopted in France and Germany. Some have agreed and some differed with my suggestion to substitute a larger unit called the "lamp" for the candle. Whether this suggestion is ultimately adopted or dismissed, I still hope that it may be possible to establish an International Unit of Light which may be called by a generally accepted name, whether the "International Candle" or the "International Lamp," or some other term; and that this convention will abolish the existing differences. At the present moment, an English 10-candle glow-lamp, even if correctly marked, is not identical with a French 10-candle or a German 10-candle lamp in actual luminous intensity, apart from errors in photometry.

Next as regards fundamental standards. I was glad to hear from Dr. Glazebrook that this matter is receiving his attention, and he will have seen from the remarks during the discussion the necessity that exists for some final standard of reference. I trust therefore it will not be long before the National Physical Laboratory will be in a position to make authoritative decisions upon this subject.

As regards working standards, many speakers have declared their preference for the amyl acetate lamp. The criticism that this lamp is less affected by barometric pressure than the pentane lamp is not, I think, valid, as it rests only on Liebenthal's experiments with an old form of pentane lamp, and not on experiments made with the present 10-candle Vernon Harcourt Gas Referee standard. There is room in this matter for further research.

With respect to my Large Bulb Glow Lamp Standards, two remarks were made which deserve attention—one by Mr. Trotter, who pointed out a possible error due to reflection from the back of the bulb. Although this objection was present to my mind, yet owing to the

method that I have adopted of using the lamps at a constant distance from the photometer, it has never been found to be a serious cause of error. But with a view to its removal, I propose to place a dead-black mica screen inside the bulb behind the filament, and this, I think, will be done in the lamps of this pattern which will be issued by the Edison and Swan United Electric Light Company.

Another useful criticism was made by Mr. J. T. Morris, who gave some figures showing the effects of a rise in the surrounding temperature on the candle-power of a lamp. This effect unquestionably exists. Since the rate of radiation of the filament depends on the difference between its own temperature and that of the enclosure, any rise in the temperature of the surrounding enclosure must diminish the rate of radiation of the filament, and therefore if the voltage on it is kept constant, the temperature of the filament will rise and its candle-power will be increased. It will be necessary, therefore, to mark on every standard lamp the temperature at which its candle-power standardisation was effected.

A large amount of discussion turned on the merits of various photometers. These are largely matters of personal preference and opinion. Some prefer a Rumford photometer, which requires binocular vision, and others a photometer such as the Lummer-Brodhun, which demands monocular vision. Some men shoot best with both eyes open and others with only one eye open. I do not think it can be laid down as an absolute rule that binocular methods are better than monocular. A person with one eye defective, but the other good, may yet do accurate photometric work. As a matter of fact, we all "see" a little more with one eye than the other. No one need allow the reading of a Lummer photometer to be vitiated by stray light if proper precautions are adopted, hence the contention made that the Rumford in that respect is not so liable to error is not based upon an inherent defect of the Lummer-Brodhun photometer.

With respect to methods of photometry, I have been accused of confusing acuteness of vision with the determination of brightness. I was under the impression that I had tried to explain this difference in my paper, and had pointed out that measurements involving acuteness of vision were different from those depending upon the sensation of brightness. At any rate I threw out a suggestion on that point, but although Sir W. Abney and Professor Ayrton have both criticised me on this point, they have not told us precisely what is the difference in their opinion. The words "luminosity" and "candle-power" are used by some writers on photometry in a very confusing manner, and it was in connection with these outstanding and difficult questions that I had hoped we might have been able to clear the air during this discussion. I purposely raised the question whether the so-called "candle-power" is a true measure of the real value of the light for visual purposes. This question has been rather evaded during the discussion. Speakers have gone off on all sorts of side issues, such as whether the commercial lamps are properly marked for candle-power, whether red and green lights can be photometered with as much accuracy as white lights, whether voltage and current can be best measured by a potentiometer or by other

instruments, which are all, no doubt, interesting matters, but they do not touch the questions lying at the basis of photometry. Dr. Fleming.

I am glad to find my statements as to the great differences between the photometric measurements of lamps by different observers are supported by Professor Ayrton, out of his large experience. The notion that lamps can be shot through a photometer room at the rate of five thousand a day and accurately photometered is, I suspect, at the bottom of a good deal of the worthlessness of much commercial marking of lamps. The manufacturer wants to get a certain watts per candle, and the candle-power is marked almost at a guess to make it fit in with the value of the marked voltage and the current. Mr. Moul thinks the potentiometer is very "pretty in theory," and confidently states that a D'Arsonval galvanometer—whatever he may mean by that—"answers every requirement" for the measurement of lamps. He may be interested to know that the potentiometers are used at the Edison and Swan factory at my suggestion, and that I was not speaking at random in recommending it as the best method for measuring lamp voltages even in a factory.

As regards actual checking of incandescent lamps for station purposes and for customers, I strongly believe that the large bulb lamps I have brought to your notice will be found very convenient; if only station engineers and others will not conclude too hastily that any voltmeter is good enough by which to set them. It seems necessary to emphasise the fact that 1 per cent. variation in the voltage of a glow-lamp implies 5 or 6 per cent. variation in the candle-power. How many commercial voltmeters taken up at random can pass a test showing them to be accurate to less than 2 per cent.? They may be accurate when new, and not always remain so.

As regards the difficult question which arises in connection with heterochromatic photometry, some of the statements in my paper have been disputed, notably one connected with the so-called Purkinje phenomenon. If Professor Ayrton is right in contending that I am wrong, then he is under an obligation to explain the reason why such authorities as Von Helmholtz, Lépinay and Nicati, Blondel and others, have made assertions which conflict with his own experiments. How are these to be reconciled? These points, however, are confessedly difficult, and I have no wish to dogmatise upon them.

The advantage of bringing a paper before this Institution is that free and kindly criticism is given, and I can only in conclusion thank the Institution for the appreciation accorded to the material gathered for their discussion, and at the same time express the feeling that the paper has been the means of drawing from a number of authorities and experts much valuable information on this subject which will no doubt be read with interest and advantage by many who were not actually present at the debate.

The PRESIDENT: Gentlemen, I put it to you that we pass a very hearty vote of thanks by acclamation to Dr. Fleming. The President.

The vote was carried by acclamation.

The President.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected :—

Associate Members.

Antonio Guitard.
Louis Tasman Reichel.
George W. Handley.

Associates.

Ralph Millar Crook.
Gerard Ogilvy Nevile.
Michael O'Sullivan, LL.B.
Alexander B. Robertson, Jun.

THE COMPANIES' ACTS, 1862 to 1900.

[COPY.]

SPECIAL RESOLUTION

(Pursuant to the Companies' Act, 1862, Sections 50 and 51)

OF

The Institution of Electrical Engineers.

Passed December 4, 1902, Confirmed December 19, 1902.

At a SPECIAL GENERAL MEETING OF THE MEMBERS ONLY of the above-named Institution, duly convened, and held at the Institution of Civil Engineers, 25, Great George Street, in the City of Westminster, on the fourth day of December, 1902, the following SPECIAL RESOLUTION was duly passed; and at a subsequent SPECIAL GENERAL MEETING of the Members only of the said Institution, also duly convened and held at the offices of the Institution, 28, Victoria Street, in the City of Westminster, on the nineteenth day of December, 1902, the following SPECIAL RESOLUTION was duly confirmed :—

RESOLUTION.

"That the Regulations contained in the Articles of Association of the Institution be altered in the following manner, that is to say :—

1. By cancelling Article 1 and substituting therefor the following :—

"1. The Articles of Association of the Institution of Electrical Engineers, as the same now exist, will remain in force up to and including the 31st day of December, 1902."

2. Article 2, by substituting "1903" for "1899," in the first and sixth lines, and "80" for "71" in the eighth line.

3. By cancelling Article 5 and substituting therefor the following :—

"5. Subject as hereinafter provided, on and after the 1st day of January, 1903, then existing Honorary Members shall continue to be Honorary Members, then existing Members shall continue to be Members, then existing Associate Members shall continue to be Associate Members, then existing Associates shall continue to be Associates, then existing Foreign Members shall continue to be Foreign Members, and then existing Students shall continue to be Students, subject to the obligations attaching to such various classes.

"The members of the different classes referred to as existing on the said 1st day of January, 1903, and such other persons as shall be admitted, in accordance with these Articles, and none others, shall be or become members of the Institution (either as Honorary Members, Members, Associate Members, Foreign Members, Associates, or Students, as the case may be), and be entered on the Register as such."

4. Article 7, by striking out the words "Foreign Members."

5. Article 12, by substituting "1902" for "1898" in the second line, and "30" for "25" in the third line, and inserting after the word "age" the words—

"*Unless the Council shall be satisfied that there are sufficient reasons for admitting him to such class at an earlier age.*"

6. Article 13, by inserting the word "new" before and the words "(*i.e.* every Associate Member not on the Register of Associate Members on the 31st December, 1902)" after the words "Associate Member" in the first line, and by striking out the word "and" in the second line, and the words "whether admitted" to the letter "(b)" inclusive.

7. By striking out Article 14 and substituting therefor the following :—

"14. *Foreign Members.* Foreign Members shall be *foreigners* residing abroad who were on the Register of Foreign Members on the 31st of December, 1902."

8. By striking out Article 16 and substituting therefor the following :—

"16. Students shall be persons of any age who, at the time of election, are serving pupilage to an Electrical Engineer or Electrician, or who are studying Electrical Science at one of the Universities, Public Colleges, Technical Institutions, or Government Schools, or who otherwise satisfy the Council that there are special circumstances which, in the opinion of the Council, entitle them to admission. No person shall remain in the Class of Students after the 31st December next following the expiration of three years from the time of his election, unless at the expiration of such three years he shall not have attained the age of 26 years, in which case he shall be entitled to remain in the Class of Students until the 31st December next following the day on which he attains that age."

9. By striking out Article 18 and substituting therefor the following :—

"18. Except as hereinafter provided every candidate for election into the Institution, otherwise than as an Honorary Member or a Student, shall be duly proposed by a Member and seconded by another Member, in each case in writing and from personal knowledge, and his candidature shall be further supported in writing by not fewer than three Members or Associate Members.

"The Secretary shall thereupon submit the application of the candidate to the Council to be considered, and if it be approved by them, it shall be brought before the next General Meeting of the Institution, with the recommendation of the Council as to the class to which the candidate should be elected, for approval. But in the event of a candidate resident abroad not being personally known to a sufficient number of Members, or Associate Members, to enable him to satisfy the foregoing conditions of proposal, if such candidate be nominated by the Local Honorary Secretary of the Country or Colony in which he resides, and if sufficient evidence be produced to satisfy the Council as to the fitness of such candidate for election to any class, the Council may propose his election to such class, and no further support will then be necessary; but the proposal form of the said candidate shall be signed by the Chairman of the meeting of Council at which his candidature was accepted, and his application shall be brought before the next General Meeting of the Institution for approval.

"Every candidate for election into the Institution as a Student shall be duly proposed, in writing and from personal knowledge, by one Member or Associate Member. The Secretary shall thereupon submit his application to the Council, and if it be approved by them, it shall be brought before the next General Meeting of the Institution for approval."

10. By striking out Article 23 and substituting therefor the following :—

"23. The Council shall decide upon the application for transferring any candidate from one class to another, but, except as hereinafter provided, every candidate for transfer to any class shall be duly nominated for such transfer, in writing and from personal knowledge, by two Members; and his candidature for transfer shall be supported, in writing, by three Members or Associate Members. But in the event of a candidate for transfer resident abroad not being personally known to a sufficient number of Members or Associate Members, to enable him to satisfy the prescribed conditions of nomination, if such candidate can produce sufficient evidence to satisfy the Council as to his fitness for admission to the class to which he seeks to be transferred, the Council may accept, without further support, the nomination of the Local Honorary Secretary of the Country or Colony in which such candidate resides."

11. Article 24, by inserting the word "and" between the words "Members" and "Associate Members," and striking out the words "and Associates."

12. By striking out Article 26 and substituting therefor the following :—

"26. On election to the Institution every Member shall pay an entrance fee of three guineas, every Associate Member an entrance fee of two guineas, and every Associate an entrance fee of two guineas. On election to the Institution a Student shall not pay an entrance fee. When a person is transferred from one class to another he shall pay the amount of the entrance fee payable by a member of the class to which he is transferred, after deducting therefrom the amount of the entrance fee (if any) paid by him, and the amount of the fees (if any) paid by him on the previous transfer, or on previous transfers."

13. By striking out Article 27 and substituting therefor the following :—

"27. Except as hereinafter provided,
"Every Member shall contribute annually to the Institution the sum of three guineas ;
"Every Associate Member shall contribute annually the sum of two guineas ;
"Every Foreign Member shall contribute annually the sum of one pound ;
"Every Associate shall contribute annually the sum of two guineas ;
"Every Student under 19 years of age at the date of election shall contribute annually the sum of one guinea up to the end of the year in which he shall attain the age of 22 years, and thereafter annually the sum of one guinea and a half. Every Student of or above the age of 19 at the date of election shall contribute annually the sum of one guinea up to the end of the third year after the year in which he was elected, and thereafter annually the sum of one guinea and a half.

"Any Member residing abroad,* or absent from the United Kingdom * of Great Britain and Ireland, the Isle of Man, and the Channel Islands, for nine months in any year, and giving previous notice in writing to the Secretary of his intended absence, shall, during the period of his absence, contribute annually the sum of two guineas. Any Associate Member or Associate so residing or absent abroad, and giving previous notice of his absence as above required, shall during the period of his absence contribute annually the sum of one guinea and a half."

* Wherever in these Articles of Association the term "United Kingdom" is hereinafter used, it is to be understood as including the United Kingdom of Great Britain and Ireland, the Isle of Man, and the Channel Islands, and the term "abroad" is to be understood as including any place situate beyond these limits.

14. Article 28, by inserting the words "while residing there" after the words "Eleven Pounds or," "Fifteen Pounds or," and "Two Pounds Ten Shillings or," respectively.

15. By striking out Article 29 and substituting therefor the following :—

"29. A Foreign Member who has not compounded shall, if he come to reside in the United Kingdom, while resident in the United Kingdom pay an annual subscription of three guineas."

16. By striking out Article 30 and substituting therefor the following :—

"30. An Associate who has compounded by payment to the Institution or its predecessors of ten pounds or of twelve pounds ten shillings, shall, if transferred to the class of Members, either directly or after passing through the class of Associate Members, pay an annual subscription of two guineas while resident in the United Kingdom, or of one guinea while resident or absent abroad as specified in Article 27 ; if transferred to the class of Associate Members he shall pay an annual subscription of one guinea while resident in the United Kingdom, or of ten shillings and sixpence while resident or absent abroad as specified in Article 27. Provided always that if such a Member compounded while an Associate and while resident abroad, and was admitted a Member before the 31st December, 1898, he shall pay no subscription while resident or absent abroad as specified in Article 27.

"Provided further, that any such Associate may compound for his annual subscription by payment of an additional composition equal to the difference between the composition he has already paid and the composition that he would have to pay if he had not compounded as an Associate.

"A Student, or an Associate, Foreign Member, or Associate Member, who has not compounded, shall, if transferred to a higher class, pay the same annual subscription as if he had been elected to such higher class on the day upon which he was transferred thereto."

17. By striking out Article 32 and substituting therefor the following :—

"32. Every Member, Associate Member, Associate, and Student, shall pay the annual subscription for the year in which he is elected, without reference to the period of the year at which his election takes place ; but he shall be entitled to receive a copy of all numbers of the "Journal" containing the proceedings of that year, and to such other publications of the Institution which may have been issued during that year, as the Council may from time to time determine."

18. By striking out Article 37 and substituting therefor the following :—

"37. The Council may in any special case where in their opinion it is desirable to do so reduce or remit the annual subscription, or the arrears of annual subscriptions, of any Member, Associate Member, Foreign Member, Associate or Student."

DUBLIN LOCAL SECTION

A HYDRO-ELECTRIC PHENOMENON.

By F. GILL, Member.

(*Paper read before the Local Section Nov.* 21, 1902.)

In June last one of the employees of the National Telephone Co. was working on a pole supporting a number of wires (17) running through a rural district near Glasgow. On touching the topmost wire the man received a severe electric shock. On reporting the matter to a member of the Engineer-in-Chief's staff, Mr. Watts, the matter was investigated, and it was found that during certain times sparks could be drawn from the wire in question. This wire was found to be an unused one, and was of copper weighing 100 lbs. per mile, about 1,000 yards long, and insulated at both ends and over its whole length. It was carried at a height of 26 feet from the ground, and its capacity would be about ·0087 m.f.d.

It was at first difficult to explain the reason for the charge as the wire seemed perfectly insulated from all other conductors. The only thing at all unusual about the line was found to be at a point where the exhaust steam from a colliery engine was blown from a distance of about 23 feet by the wind against the wires. The exhaust pipe extended vertically 18 feet and was 3 inches diameter at the top. It was found that the charge only occurred when the engine was working on load, and only when the wind blew the exhaust steam against the wire. Further investigation was made by Mr. Watts, who had a collector constructed of a long bamboo rod with an insulator at the top on which was fixed a number of short pieces of wire with a V.I.R. covered wire connected as a lead to the ground. When this collector was held in the steam near the mouth of the iron exhaust pipe a series of sparks was obtained from the covered lead, and the origin of the charge completely located. The weather during these experiments was exceptionally dry, and the charge could not be obtained on damp days.

Since that date Professor Magnus Maclean of Glasgow, having had his attention drawn to the phenomenon by Mr. Valentine, the Company's District Manager, has conducted some experiments by means of a portable electrometer. He found that when a similar collector to that already mentioned, but with the lead wire terminating above the ground, was inserted near the steam, continuous sparks of from $\frac{1}{2}$ in. to $\frac{3}{4}$ in. long could be obtained between the end of the lead and a metal rod driven into the ground, the best results being obtained when the pressure of the escaping steam was highest. This length of spark would indicate a potential difference of about 40,000 volts. Professor Maclean also took sparks from the lead through his body and noticed that the physical pain experienced was much more severe than from a spark due

t 100,000 volts' pressure derived from a large 24-plate Wimshurst e.

:essor Maclean also tested the potential of the air 6 feet above the and found when under the issuing steam and about 12 feet from 1,100 volts, and at a point 30 feet from the steam about 900 volts, ıe engine was working. The electricity generated was positive : case. Of course, the hydro-electric effect of steam under e is well known and has been investigated by Faraday and by ong, but it is very seldom that the effects are seen in the natural f things.

MANCHESTER LOCAL SECTION.

HIGH TEMPERATURE ELECTRO-CHEMISTRY: NOTES ON EXPERIMENTAL AND TECHNICAL ELECTRIC FURNACES.

By R. S. HUTTON, M.Sc., Associate, and J. E. PETAVEL, Associate Member.

(Paper read at Meeting of Section, November 25, 1902.)

Although a few pioneers like Siemens and Cowles foresaw the importance of the application of the electric furnace to chemical problems, it is only within the last ten years that most of the important processes have been developed. With the discovery of calcium carbide in 1892, the commercial possibilities of making use of the extreme temperature produced in the electric arc seem to have first forcibly impressed themselves both on the chemist and engineer, and the demands for power thus created brought into existence all over the world large generating plants of which Niagara is a typical instance. In the early days Cowles found great difficulty in obtaining an electric plant of sufficient power for his purposes,[1] but soon the electrical engineer, realising the nature of the demands made upon him, was fully able to cope with them. The provision of cheap power has in turn reacted in stimulating the development of many new chemical industries. The magnitude of the present development of electric power for chemical purposes is clearly brought out in the statistics published by Swan.[2] It is usual to explain the very small progress of electro-chemistry here as compared with other countries by invoking the well-worn excuse that comparatively little water power is available in Great Britain. This subject is worthy of much closer attention than has been given it in the past. In the first place it is well to remember that in a great number of cases the cost of power is only a small percentage of the prime cost of the manufactured material. Again, the water power is frequently most inaccessibly situated, thus raising very considerably the cost in freight on raw material and finished product.[3] Owing to the improved efficiency of the steam engine and to the huge progress made recently in the application of the gas engine to large powers, the cost of energy derived from coal is daily

[1] Crompton, *British Assoc. Reports*, p. 809 (1888).

[2] *Journal Soc. Chem. Industry*, vol. 20, pp. 662–676 (1901). See also Borchers, *Die Elektrochemie auf der Weltausstellung in Paris*, 1900, Halle a. S.

[3] Report of Arrhenius to Swedish Government, see *Electrician*, vol. 47 p. 71 (1901).

becoming less,[1] and is already capable of competing successfully with the less favourably situated water powers.

There is, indeed, no valid reason why many of the electro-chemical industries should not be a success in this country. As will be seen later, a considerable number of electric furnace products are absorbed directly by the iron and steel industries, which of all manufactures in this country are probably the most favourably situated for obtaining cheap power. The manufacture of the alloys of the rarer metals, either in direct connection with some existing steel works, or at any rate in these districts, should offer every economical advantage; moreover, in these cases the raw product forms a very large proportion of the total cost. Much has been written of the future which has been opened up by the application of producer and blast furnace gases,[2] but even if we were to consecrate our whole time to the importance of this subject in its bearings on the electro-chemical industry, we could hardly do it justice. We will therefore pass directly, first to the consideration of the subject in its experimental stage, and then follow it in some of its commercial applications.

PART I.

EXPERIMENTAL EQUIPMENT.

In considering the equipment of a laboratory for experimental work in Electro-Metallurgy, the point of utmost importance should be, in the first place, to provide power to enable the experiments to be carried out on a reasonably large scale. The actual magnitude of the power equipment must be regulated by two principles. It is very desirable that the experiments though not on a scale directly comparable with the commercial process should, nevertheless, be of sufficient magnitude to furnish reliable practical data. On the other hand, as the question of cost has unfortunately to be considered, it is necessary to keep the equipment within certain limits, so that any given experiment can be repeated frequently under all possible conditions. Such work will supply not only valuable scientific information, but also the necessary data for practical application. Having said thus much with regard to the scale of the work, let us consider what should be the main points governing the choice of equipment. As we shall see later, the variety of different forms of electric furnaces which have been proposed or used is extremely great, and it would be altogether impossible to provide in any laboratory, however large or wealthy, even the most important of these. With regard to the generating plant the same may be said. In commercial work we find conditions varying from the 15,000-volt nitric acid plant down to the 4 or 5 volts required by the zinc or aluminium processes; from the continuous current used in all electrolytic work, to the mono- or multi-phase alternating current employed

[1] Humphrey, *Proc. Inst. Mech. Engineers*, 1901, pts. i. and ii., p. 41; *Brit. Assoc., Sect. G., Belfast* (1902).

[2] Bryan Donkin, *Min. and Proc. Instit. Civil Engineers*, vol. 148, pp. 1–55 (1902).

design of the water-tube frame will therefore depend on the dimensions of the tube and the available head of water. As a general guiding factor we may say that a flow of one litre per minute will dissipate up to 2½ k.w.[1]

Close to the water-tubes a resistance is provided by which the exciting current of any of the dynamos can be most effectively

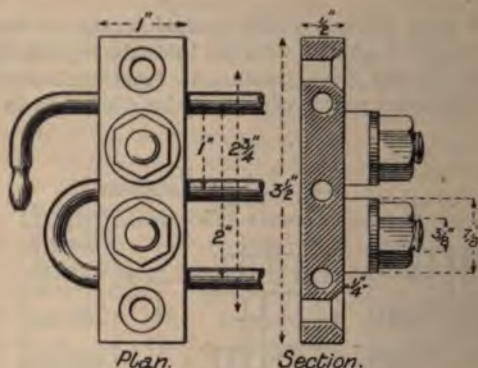

FIG. 3.—Terminals of Water Tube Resistance. (600 Ampere Frame.)

The German-silver tubes are soldered into gun-metal strip carrying two ⅜-inch bolts. The water passes in series through several tubes, connections being made by means of a U tube as shown in the figure. The gun-metal strips are mounted on wooden frames.

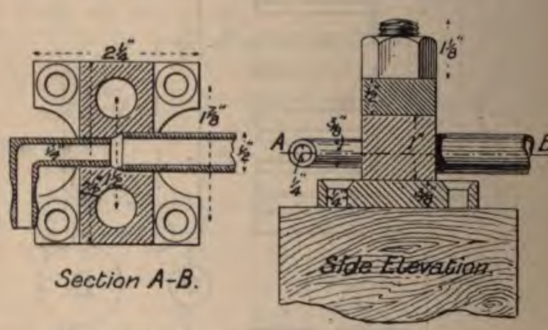

FIG. 4.—Terminals of Water Tube Resistance. (1,000 Ampere Frame.)

Gun-metal castings form the mounting for the German-silver tube, and carry two ½-inch bolts by which the connections are made.

regulated; it is in constant use for the considerable variations of voltage necessary during the progress of some of the furnace operations. We now come to the actual furnace equipment and the different types of apparatus which are best adapted for laboratory work. Foremost

[1] The larger tubes are distinctly the more satisfactory, since in this case there is no difficulty in obtaining an efficient flow of water from the ordinary supply. With the smaller tubes it is necessary to have numerous inlets and outlets, as their resistance to the flow of water is considerable.

FIG. 2.—Regulating Resistance fitted for 40-kw. Moissan Furnace.

To the right is the portable resistance, and above the 500, 600, and 1,000 ampere short-circuiting switches.
2 × ½ inches connect these switches

amongst these must be placed the Moissan furnace.[1] A dimensioned drawing of this type for a power of about 40 k.w. is given in Fig. 6. Taking into consideration the many discoveries made by Moissan in the course of his investigations, it is surprising that as yet only a very small percentage have found commercial application. The reason of this may, perhaps, be ascribed to the fact that this form of furnace, though proving itself eminently suitable for the pioneering work for which it was intended, scarcely gives any data on which a technical process could be founded. It is, in fact, a striking example of how the apparatus most suited for purely scientific work is seldom capable of direct commercial application. It was not until the work was taken up by the practical engineer that the scientific discovery developed into a commercial industry. We shall see below that most frequently the financial success of a process has been in direct proportion to the mechanical improvements introduced, the chemical modifications being generally of secondary importance. It is therefore necessary to be able to provide a type of furnace corresponding in principle to the most usual technical forms, and thus carry out the experiments, if not on the same scale, at least in the same manner as would be done in the factory. Fig. 7 is a plan of an apparatus similar to that used by Haber,[2] which we have found extremely useful for representing many different forms of furnace. Connected as shown in Fig. 9A, it has proved itself most satisfactory for the manufacture of aluminium by the electrolysis of cryolite containing Al_2O_3, replacing the carbon block *C* shown in this figure by a small furnace built of loose bricks, a good example of a carbide "pot" furnace giving a most satisfactory yield of calcium carbide can be obtained. Frequently in the electrolysis of fused salts it is an absolute necessity to make the crucible lining of the material itself. This result is obtained (as shown by Muthmann, Hofer and Weiss),[3] by the use of a water-cooled crucible. The arrangement is shown in Fig. 9B. Whilst the vertical holder enables us to represent a large number of different types of furnace, those of the Acheson Carborundum type can be very simply built up by the use of ordinary materials. Fig. 10 shows a carborundum furnace as used with 40 H.P.

The next figure (Fig. 11) is a drawing of a furnace suitable for making calcium carbide or for other furnace operations. The design is inexpensive, and the apparatus most serviceable.

Such experimental equipment will be of importance not only from an educational point of view, but it is by no means impossible that certain of the difficulties in technical processes may be overcome by its use. It is important, however, not to neglect the purely scientific work, which, though it may possibly not find any direct application in the immediate future, has, however, frequently proved to be the starting-point of notable advances. In this direction the work in progress consists in the determination of the effect of gaseous pressures on high temperature chemical phenomena ; it is proposed to study care-

[1] Moissan, *Le Four Électrique*, Paris, 1897.
[2] Haber, *Zeitschr. für Elektrochemie*, vol. 8, pp. 1, 26, 607 (1902).
[3] *Leibig's Annalen*, vol. 320, pp. 231–269 (1902).

fully some of the gaseous and other reactions which may be expected to differ considerably from those occurring under ordinary conditions. The apparatus shown in Fig. 12 is destined for work up to 200 atmospheres, and for currents up to 1,000 amperes, and has been provided out of funds received from the Government Grant Committee of the Royal Society.

PART II.

NOTES ON TECHNICAL PROCESSES.

In the comparatively short time which is at our disposal it would be altogether impossible to give any adequate account of the general development of the industry. We shall, therefore, neither touch on the historical side of the subject, which has already been fully treated by others, nor do we propose to give a complete account of the methods

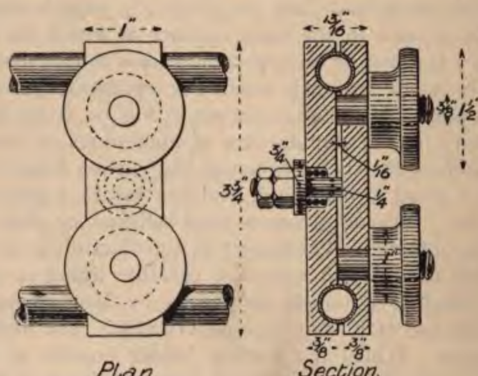

FIG. 5.—Sliders for 1,000-Ampere Water Tube Resistance.

These are used to regulate the larger currents, one being moved a short distance whilst the other remains fixed, this latter being in turn advanced. The E.M.F. on the moving slider is thus kept small and sparking avoided.

in use in such widely developed industries as calcium carbide, aluminium, etc. Our object will be more especially to consider some of the newer industries, to draw attention to recent advances in the older ones, and to consider where possible, the directions in which improvements are to be expected. In connection with each subject a few references are given which may be of interest to those desiring more complete information. In order to facilitate the description, a diagrammatic representation of the principal types of electric furnaces is given in Fig. 13.

CALCIUM CARBIDE.[1]—We have not here to deal with the economical

[1] Vivian B. Lewes, *Acetylene*, London, 1900; Moissan, *Comptes Rendus*, *vol.* 115, 1031 (1892), vol. 118, 501 (1894); Moissan, General Review of Chemistry of Carbides, *Rev. Générale des Sciences*, vol. 12, pp. 946–955 (1901).

side of the subject, but it may be said that the financial crisis through which the carbide industry has recently passed influenced very considerably its progress.[1] Previously the manufacture of carbide was being taken up in innumerable small factories with processes differing very largely as to their efficiency. The present circumstances have eliminated the less efficient methods, and the comparatively few firms which are still successful owe their advantage to the careful consideration they have given to the perfection of the mechanical details. Speaking generally, it may be said that the tendency, here as in other manufactures, has been to simplify the process as much as possible. The current regulation is automatic,[2] and is achieved either by raising the vertical electrodes when the "pot" type of furnaces are used, or by some other means of bringing more resisting material between the electrodes. Means are frequently provided for automatic grinding, weighing, and mixing of materials. With regard to the labour-saving problem, a continuous furnace possesses obvious advantages. A great deal of discussion has arisen with regard to the relative merits of the "continuous" and "discontinuous" furnaces, but this is largely due to a confusion of terms, since the tapping methods,[3] which undoubtedly give a poorer grade carbide, are by no means the only continuous ones; in fact with such furnaces as the Horry, Siemens and Halske, etc., all the advantages of a continuous process are attained, without the great loss of heat which is entailed by the removal of the molten carbide from the furnace. The production of calcium carbide being a purely electro-thermal operation, either continuous or alternating current can be used, but the balance of advantage seems to be decidedly in favour of alternating current, which lends itself more easily to long-distance transmission. An alternating plant is, moreover, more suitable to withstand the sudden and intense variations in load, which it is at times impossible to avoid, and the carbide produced is said to be more uniform in quality.[4]

Three-phase current, which has been employed notably at St. Marcello d'Aosta,[5] has the advantage of giving a more even distribution of temperature. It must be remembered that the most favourable temperature conditions are somewhat narrow,[6] and that too intense a heat can produce the phenomenon of "burning." Alternating current has, however, one serious disadvantage, namely, that the power-factor is somewhat low. Experience has shown that with a view to preventing both the inconvenience and loss caused by the dissipation of the finely divided material, it is necessary to have the furnace properly enclosed.

[1] A careful estimate recently made points out that of the 254,000 H.P. installed for this manufacture, only some 64,000 H.P. are in use. *Mineral Industry*, vol. 10, p. 74 (1902).

[2] *E.g.* Horry, *U. S. Patent* 655,779 of 1900.

[3] Carlson, *Zeitschr. für Elektrochemie*, vol. 6, pp. 413, 429 (1900); Frölich, *Zeitschr. für Elektrochemie*, vol. 7, pp. 1–10 (1900).

[4] Pradon, *Electrical Review*, vol. 49, p. 463 (1901).

[5] Cesare Pio, *Electrician*, vol. 43, p. 637 (1899); *Electrical World and Engineer*, vol. 40, p. 375 (1902); Bertolus, *Engl. Patent* No. 16,942 (1897).

[6] Rothmund, *Zeitschr. für anorganische Chemie*, vol. 31, p. 136 (1902); Borchers, *Zeitschr. für Elektrochemie*, vol. 8, p. 349 (1902).

This minimises to a large extent the oxidation of the carbon electrodes, which is also an important question with regard to cost, and further enables the gases to be made use of for heating purposes. There seems to be a distinct gain to be achieved by finely dividing and intimately mixing the raw materials, and in modern enclosed furnaces these appear now to give very little trouble.

The carbide industry has reached a large development in the United States, where at Niagara some 15,000 H.P. are continuously in use. Although the distance of these works from the power-house is only about two miles, it has been found economical to transform the 2-phase current generated at 2,200 volts, to 3-phase at 11,000 volts, the trans-

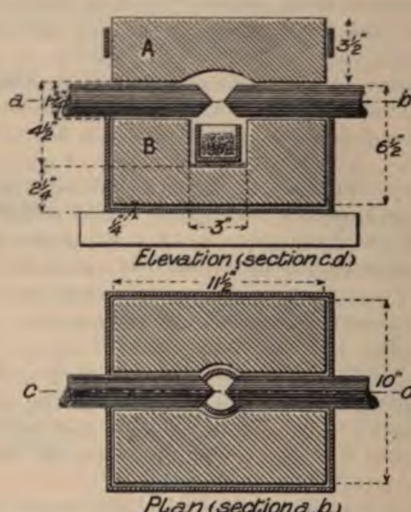

FIG. 6.—40-k.w. Moissan Furnace.

This is constructed of blocks of Monk's Park Bath Stone, which after trials with various limestones has proved to be the most suitable. The furnace is composed of two blocks, the first A, forming the cover, the second B, the furnace proper. This latter has two grooves of slightly greater diameter than the carbons to be used. As the blocks are apt to split under the influence of the heat, they are held together by iron bands; these are provided with sockets (not shown in figure) which enable the furnace or cover to be lifted by long iron bars. The central hole which may be of diameter 2½–4 inches, according to size of crucible in use, is lined at the bottom by a layer of powdered magnesia which prevents the carbon crucible coming in direct contact with the lime. An annular space should be left around the crucible, which not only facilitates the heating, but also prevents the lime and carbon from entering into combination.

formers being Scott connected and of capacity 2,500 H.P. At this pressure the current is received at the transformer-house close to the carbide works and transformed down, first to 2-phase 2,200 volts, then to 110 volts, at which it enters the furnaces.[1] At Saulte Ste. Marie, a sister factory for 20,000 H.P. is nearly complete. A plan of the Horry

[1] *Electrical World and Eng.*, vol. 34, p. 794 (1899).

furnace given in Fig. 15 is of some interest, this being the type of furnace employed in these works.[1]

Of other furnaces, those of Gin and Leleux[2] and the Deutsche Gold u. Silber Scheide Anstalt[3] are amongst the most widely known, the former having been fitted up at Meran and in some parts of France and Italy, whilst the German firm have equipped several factories in Norway and elsewhere. The condition of the carbide industry is at the present time entirely dependent on the progress of acetylene lighting, and despite the unfortunate reaction caused by the flooding of the market by improperly constructed generators, now that the subject has received due attention and safe and reliable apparatus are available,[4] steady progress is being made.

Possibly the application of acetone as a solvent for acetylene,[5] enabling it to be safely stored under pressure, may influence its future considerably.

Other proposals for the use of calcium carbide, which have not as yet come into general use, include its application as a metallurgical reducing agent,[6] and for the production of the finer grades of lamp-black.[7]

With regard to the efficiency of the manufacture, an important advance would undoubtedly result from any invention enabling the heating power of the waste gases to be more generally used for preliminary treatment of the raw materials. The combination of the lime kiln and the electric furnace would lead to a great economy of energy, but up to the present the practical difficulties have outweighed the theoretical advantage.

Refractory Metals and their Alloys.—The difficulties in the way of the commercial application of the electric furnace having been successfully overcome in the case of calcium carbide, efforts were soon directed to the possibilities of producing some of the rarer metals on a large scale in a similar type of furnace. The valuable qualities of some of these metals in the manufacture of steels insured their finding a satisfactory market. Amongst these metals Ferro-Chromium takes an important place, chiefly on account of the large employment of chrome steels for the manufacture of armour plate, projectiles, tool steels, springs, etc. At the present time one factory alone in America is using 450 tons of 70 per cent. ferro-chromium a year for armour-plate work. For this country data are not easy to obtain, but doubtless, owing to the advanced state of the British steel trade, large quantities of these alloys are employed. The chief factories are situated in France and the United States. The Willson Aluminium Company, with works at Holcombs Rock, Va. and Kanawha Falls, W. Va., manufacture ferro-

[1] Horry, *English Patents* No. 22,521, 1897, and No. 14,261, 1899.

[2] Borchers, *Zeitschr. für Elektrochemie*, vol. 7, p. 236 (1900); Haber, *Zeitschr. für angew. Chemie* (1901), p. 185.

[3] Kershaw, *Electrician*, vol. 46, p. 267 (1900).

[4] *Report of Committee on Acetylene Generators* (Home Office), 1902.

[5] Fouché, *Soc. Française de Physique*, No. 171 (Nov. 15, 1901); *Journal de l'Electrolyse*, vol. 10, No. 122, p. 13 (1901).

[6] v. Kügelgen, *Zeitschr. für Elektrochemie*, vol. 7, pp. 541, 557, 573 (1901).

[7] Hubou, *Mémoires de la Société des Ingenieurs Civils de France* (1900).

chromium and other alloys, using some 3,000 H.P. at the latter works; the furnaces they employ are suitable for tapping, the metals being run into lined iron trucks, the automatic regulation as used in the carbide industry being also installed. Tungsten and ferro-tungsten, which have been used for a considerable time for manufacturing self-hardening and high-speed tool steel, can also be most satisfactorily manufactured in the electric furnace, and this product has considerable advantage in that it is in a compact fused form, and is thus less liable to oxidation in the process of adding to the steel. One of the most important of these alloys is Ferro-Silicon, the manufacture of which has been taken

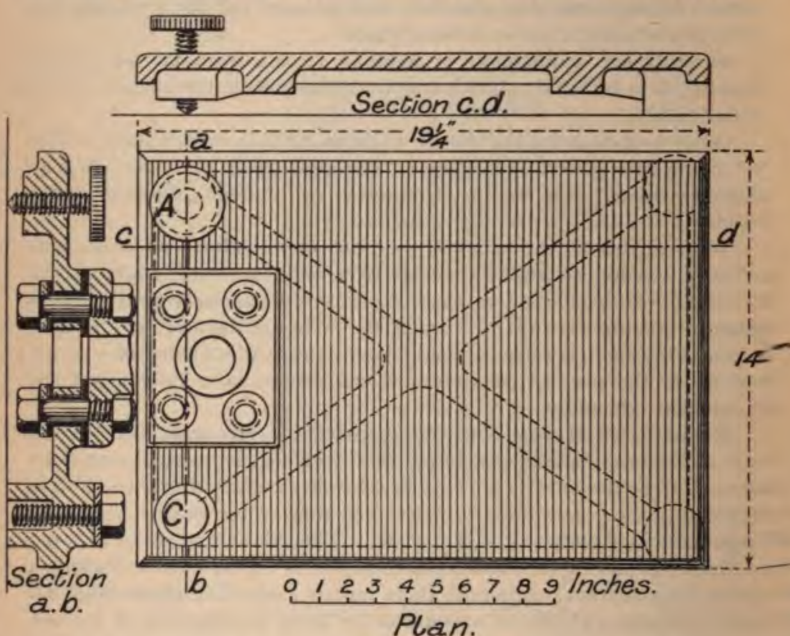

FIG. 7.—Vertical Electric Furnace Plan.

The apparatus consists of a cast-iron base, an adjustable standard, and a feeding gear. The base is $\frac{1}{2}$ inch ruling thickness, strengthened with rims and cross-ribs 1 inch thick. At one corner a $\frac{3}{4}$-inch bolt C serves as a terminal. A is a levelling screw. The square base of the standard is insulated by a slab of vulcanised fibre, and held down by four $\frac{3}{4}$-inch bolts. See section A–B.

up in several of the factories in which calcium carbide can be no longer produced with sufficient economy. The grade varies considerably, from 15 per cent. up to 85 per cent. The chief application of ferro-silicon is in the casting of iron and steel, where it plays the important *rôle* of deoxidiser, thus getting rid of blow-holes, and at the same time, owing to the high heat of combustion, renders the metal more fluid for casting.

As to Copper Silicon, this has been prepared for a long time in the furnace of Cowles, and has found considerable application as a *deoxidiser,* and for increasing the tensile strength in copper and brass

castings. There seems every indication that Silicon itself will soon displace its alloys for some of the more important work, it is now prepared in considerable quantities by the method of Scheid.[1] Among the rarer metals which have not as yet received any wide technical application, we may mention Ferro-Titanium and Ferro-Vanadium. Owing to recent work these metals have been shown to have considerable technical value in the steel industry, and the difficulties connected with their manufacture have been largely overcome. According to Rossi, the addition of titanium to pig-iron of whatever quality produces a considerable increase both in tensile and transverse strength.[2] A similar but less marked effect is produced on steel. It is evident that when dealing with raw material of such high value as is the case with some of these rarer metals, it is very necessary to have an electric furnace in which the process can be kept well under control, and in which the loss of material can be minimised as much as possible; this result has been achieved by using a properly closed furnace. The development of the industry of the rarer metals has been most materially assisted by the beautiful discovery of Dr. H. Goldschmidt in which the metallic oxides are reduced by finely divided aluminium; the process is worked at Essen, by the Allgemeine Thermit-Gesellschaft. This method at first sight possesses advantages over the direct electric furnace treatment, since metals with very low carbon content can be easily prepared; but the refining of the metals in the electric furnace is by no means impossible of accomplishment,[3] and will doubtless come more into general use and be further worked out as the industry increases. On the other hand, the Goldschmidt method is so indirect —aluminium, itself an expensive electric furnace product, being used— that, provided a sufficient demand for any given metal exists, no trouble should be found in overcoming the few difficulties which remain for preparing a sufficiently pure material, by a much more economical and more direct method.

CARBORUNDUM.—This industry, although by no means to be compared with calcium carbide so far as power is concerned, is of considerable importance and illustrates a type of furnace which is finding many other applications. A photograph will be found in the early part of the paper, Fig. 10, which, though on a smaller scale, gives a very good idea of the general appearance of such a furnace. The industry has increased very rapidly, for whereas in 1893, 6¾ tons were manufactured, in 1901 1,690 tons were produced, and the power in use has just been increased (September, 1902) from 2,000 H.P. to 3,000 H.P., thus raising the output to some 2,690 tons. The manufacture of carborundum has often been described.[4] The raw materials which are heated together in a furnace of type E, Fig. 13, consist of coke and sand; a small percentage of sawdust and salt is added, the one to insure the porosity of the charge, the other to act as a flux. Carborundum, as is well known, has found large use as an

[1] Scheid, *Engl. Patent* No. 18,659 of 1899.

[2] Rossi, *Mineral Industry*, vol. 9, p. 715 (1900); Goldschmidt, *Zeitschr. für angew. Chemie*, Heft 28 (1902).

[3] Moissan, Refining Chromium, *Le Four Électrique*, p. 209; see also *Chemical Trade Journal*, vol. 30, p. 453 (1902).

[4] Kohn, *Journ. Soc. Chem. Industry*, vol. 16, p. 863 (1897).

abraisive, but of still more interest is its recent application in the steel industry, replacing to a considerable extent ferro-silicon. The importance of this development may be judged by the fact that the present consumption of carborundum for this purpose alone is 75 tons per month. Still more promise is shown by the proposed use of this substance for making highly refractory materials. In this connection the discovery of Fitzgerald of "recrystallised" carborundum, which is prepared by agglomerating the finely divided material and reheating in the electric furnace, should be noted, as also that of Tone for using for similar purposes the "amorphous" variety, which always forms a considerable proportion of the product. Each of the carborundum

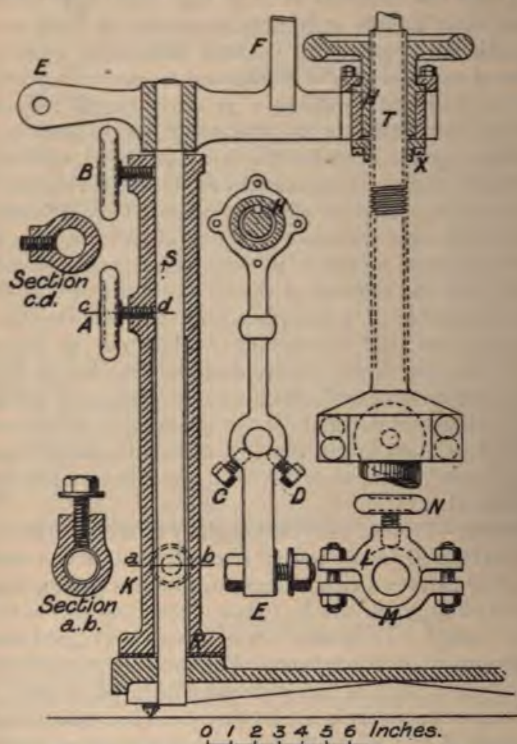

FIG. 8.—Vertical Electric Furnace Elevation.

A hollow cast-iron column R, $2\frac{1}{2}$ inches diameter, is fixed on to the base shown in the previous figure but insulated from it, the height of the gun-metal cross-bar carrying the screw feeding gear can be adjusted within wide limits by means of the $1\frac{1}{2}$-inch steel rod S which is clamped in position by the hand wheels A and B. Connection can be made at either of the terminals K or E (the latter for currents above 600 amperes), the opposite pole being connected to the base. To avoid sliding contacts which would be objectionable for such large currents connection from the cross-bar to the screw carrying the carbon rod is made by means of four $\frac{1}{2}$-inch flexibles not shown in this figure, but which can be clearly seen in Fig. 9. The gun-metal rod T, which carries the carbon, is $1\frac{1}{2}$ inches diameter with a square screw thread (4 threads to the inch), giving a feed of about 1 foot; it is raised or lowered by means of a 7-inch cast-iron hand wheel. The design of the carbon holder is clearly shown, it is provided with rings of various diameter, so that carbons of very different sizes up to 3 inches can be held.

A B

FIG. 9.—A. Experimental Aluminium Furnace.

This shows one way in which the vertical furnace, the design of which is shown in the previous figures, can be used. Upon the cast-iron base which forms the negative pole is placed a large block of carbon, having a cavity which serves as a crucible. The positive electrode is formed by a carbon 3 ins. in diameter, which is fed by the screw-gear described above.

B. Water-cooled Electrolytic Furnace.

Here the apparatus is fitted to be used for the electrolysis of fused salts; the water-jacket, by causing a layer of the fused mass to solidify, enables the electrolyte itself to form the crucible lining. This is frequently necessary, since with many substances it is impossible to find a furnace material capable of withstanding their corrosive action at the high temperatures which prevail. The negative pole is in this case formed by a vertical carbon, which is held in a clamp sliding on a gun-metal vertical rod fixed to the base.

FIG. 10.—Experimental Carborundum Furnace.

furnaces in use at Niagara employs 1,000 H.P., the voltage starting at 200, and falling to 80 as the furnace heats up; the units are kept working at full power, the voltage being varied according to the progress of the reaction.

As is well known, the power is distributed by the Niagara Falls Power Company on the constant-pressure system, and it is therefore necessary to have some means by which this variation in the resistance of the furnace can be coped with. The two principal ways in which this can be accomplished are to change the ratio of the transformer, or to add in another small transformer which can be used as a booster; in either case it is necessary to deal with the primary, as the secondary connection for such large currents cannot easily be manipulated. Fig. 15 shows two of the most satisfactory ways of connecting for this purpose.[1]

ARTIFICIAL GRAPHITE.[2]—It was during the development of the carborundum industry that Acheson's attention was drawn to the formation of graphite in his resistance furnace, a discovery which has since been applied on a large scale. Artificial graphite had been known since the work of Despretz in 1849, and from a commercial point of view had been produced by the Girard and Street process, which consists in passing the amorphous carbon through the electric arc. The Acheson process, however, is capable of dealing with the material in larger bulk and gives a pure graphite, containing as little as one-tenth per cent. of ash. At first the work was limited to graphitising articles of agglomerated amorphous carbon. Such graphite electrodes are quite indispensable for the success of many of the aqueous electrolytic processes, both on account of their more compact nature and of their greater stability.[3]

It is generally considered that the presence of small quantities of metallic oxides is indispensable for the graphitisation of amorphous carbon; in fact Acheson has found that pure carbon submitted to the highest temperatures of his furnace remains untransformed. He has recently discovered, however, that there is no need for an intimate mixture of the carbon and metallic oxide, since the reaction can take place by a kind of cementation process if the two are roughly mixed together, the metal vapour easily permeating the material. The action appears to be a catalytic one, caused by the progressive formation and dissociation of metallic carbides, the presence of a small percentage of impurity thus being used time after time in the reaction. Girard and Street already in 1895 pointed out the part played by the metallic oxides in this reaction; they obtained, moreover, a fairly complete trans-

[1] Peck, *Electrochemical Industry*, vol. 1, p. 5 (1902).

[2] Despretz, *Comptes Rendus*, vol. 29, p. 709 (1849); Berthelot, *Ann. de Chim. et de Phys.* (iv.), vol. 19, p. 392; H. Moissan, *Le Four Électrique*, pp. 85–111; Girard and Street, *Engl. Patent* No. 13,340 of 1893, *German Patent* No. 78,926 of 1893; Street, *Soc. Intern. des Electriciens*, see *Electrician*, vol. 35, p. 542 (1895); E. G. Acheson, *U.S.A. Patent* No. 568,323 (1896); F. A. G. Fitzgerald. *Journal Soc. Chem. Industry*, vol. 20, p. 443 (1901); also *Journal of Franklin Institute*, Dec. 11, 1896; Borchers, *Zeitschr. für Elektrochemie*, vol. 3, p. 393 (1897).

[3] Sprösser, *Zeitschr. für Elektrochemie*, vol. 7, p. 971, etc. (1901); Förster, *Zeitschr. für Elektrochemie*, vol. 8, p. 143 (1902).

formation of the amorphous carbon into graphite. Moissan, who has minutely studied the formation of graphite, has found that its properties vary largely according to the method of production; but the question is by no means exhausted, and it seems probable, since the requirements vary from case to case, that a careful study of the influence of individual impurities upon the graphite produced, may lead to the preparation of electrodes still more suited for any particular electrolysis than those at present manufactured. More recently this method has been applied to the direct graphitisation of anthracite coals, with the production of a granular graphite, possessing very valuable properties for

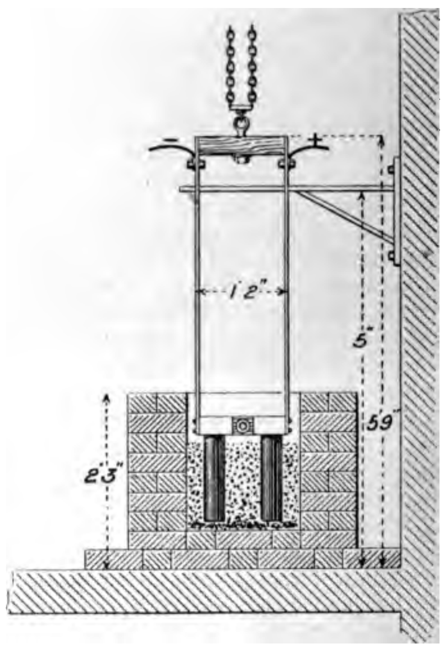

FIG. 11.—40-k.w. Experimental Carbide Furnace.

This represents a satisfactory and convenient laboratory form of the many and important technical furnaces of type C, Fig. 13. The two carbons, which are clamped side by side in a single holder but insulated one from another, are raised and lowered by means of a crane fitted to the wall. The current is led in by flat copper bars, which by sliding in grooves prevent any oscillation or side motion of the apparatus. The body of the furnace consists either as shown in figure, of fire bricks, or of a large cast-iron pot; in both cases the material itself forms the actual lining.

lubricating and similar purposes. Fig. 16 gives section of the furnaces used for graphitising electrodes. So as to increase the resistance of the furnace and reduce the intensity of current necessary, the electrodes are laid transversely and are surrounded by a mixture of coke and carborundum. On the other hand, the resistance of the anthracite being initially high, a small core of carbon is inserted. The resistance of the furnace falls with the progress of the reaction, but is

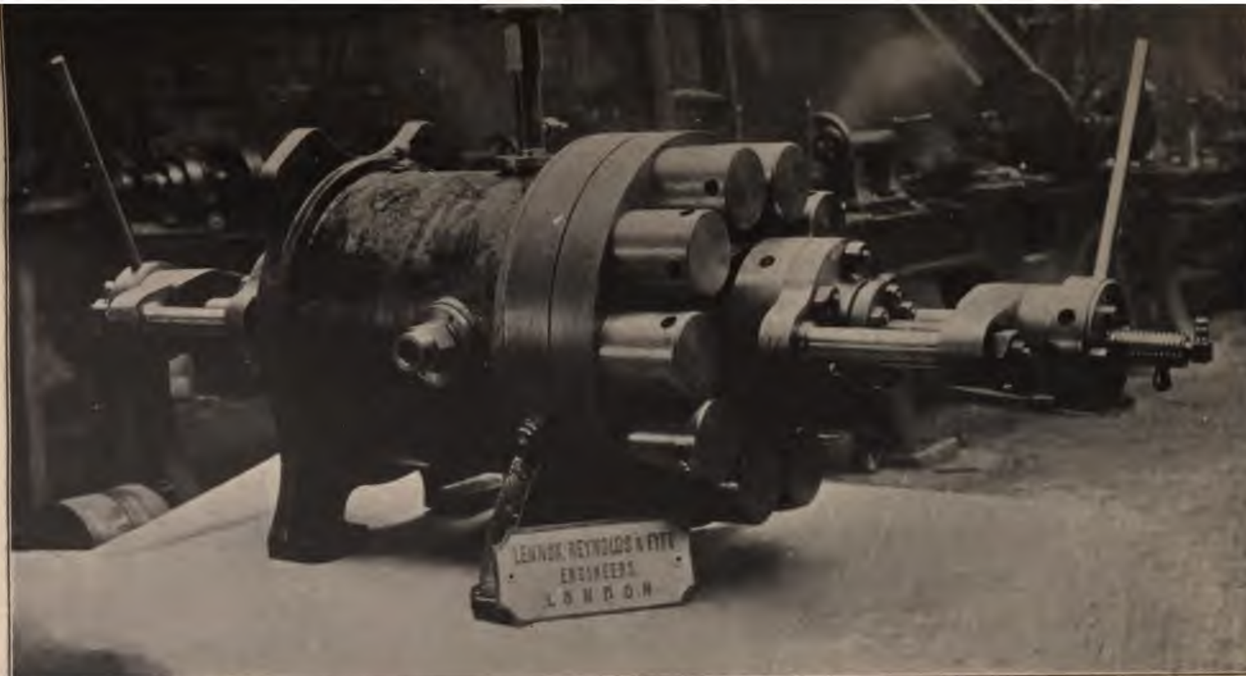

FIG. 12.—High-pressure Electric Furnace.

The apparatus is practically a steel enclosure, placed either horizontally or vertically, which can be used for any of the different types of furnace. It is air-tight, and designed for a working pressure of 200 atmospheres, being tested at 500 atmospheres. On the right and left will be seen the screw-gear by which the carbons are fed, whilst in front and behind are windows which will stand the full pressure, but which can be replaced by additional connections when a circulation of gas through the apparatus is desired. The main valve, which serves also to connect the pressure gauges, will be seen on the top. The apparatus is water-jacketed throughout, the refractory material of which the furnace proper is made being contained in a cast-iron lining, thus insuring the protection of the main forging. The construction of this apparatus has been carried out by Messrs. Lennox, Reynolds and Fyfe to the designs made at the College.

of course lower than in a carborundum furnace; the system of regulation is similar. The transformers first installed gave up to 37,000 amperes at between 30 and 15 volts on their secondary circuit, the leads being properly interlaced to bring up the power-factor to a maximum. The frequency used at Niagara is as low as 25 $\sim$; it is probable, however, that in installing a generating plant solely for such chemical work, an even lower frequency would be preferred. By the graphitising process the conductivity is increased some fourfold; the density is also changed, rising from 1·5–1·9 that of amorphous carbon, to 2·1–2·25 that of graphite. Apart from the question of conductivity an important advantage of graphite is the ease with which it can be machined. It can be cut and planed with ordinary wood tools, and screwed without any difficulty. The American production, starting with 162,382 lbs. in 1897, has increased to 2,500,000 lbs. in 1901, about half this being in the form of electrodes. 1,000 H.P. are employed in this manufacture, a number of furnaces being in use, each one in turn taking the full power; the change over can be made in a few minutes.

We now pass to the consideration of aluminium, zinc, sodium, caustic soda, etc., which are primarily electrolytic processes, the heating effect of the current, though used to keep the cell in the molten condition, being of secondary importance. The electrolytic bath could of course in all cases be easily and more cheaply kept molten by means of an ordinary furnace, but these electrolytes are generally so corrosive in their action, that the wear and tear on the crucible would make such a process unworkable. The electrolyte when fused by the current itself usually remains solid around the walls of the furnace, and thus forms a protective layer.

ALUMINIUM.[1]—Since 1889 the only two processes in actual use for preparing this metal are those of Héroult and Hall, the former being confined to Europe, the latter to America. These two methods, the chemical and electrical nature of which seem to differ very little at the present time, consist essentially in the electrolysis of fused cryolite, to which Al_2O_3 is added as the separation of aluminium proceeds.

The type of furnace used in either case is that of D, Fig. 13, the carbon-lined crucible forming one electrode, the other being made up of a number of separate carbon rods. The differences between the two methods are essentially mechanical in nature; it is not easy to obtain any detailed information, as no doubt the success of the process depends in each case upon the perfection with which some of the inherent difficulties have been overcome. The Hall process is worked by the Pittsburg Reduction Company, who have the largest output of this metal. At Niagara Falls, in the two works 10,000 H.P. are used; at Shawinigan Falls some 5,000 H.P.; whilst at Massena, N.Y., a new plant is being erected for 12,000 H.P. The cells in use consist of thickly

[1] Wallace, *Journal Soc. Chem. Industry*, vol. 17, p. 308 (1898); Becker, *Manuel d'Electrochimie*, Paris, p. 175 (1898); *Mineral Industry*, p. 14 (1892); Chandler, *Journal Soc. Chem. Industry*, vol. 19, p. 609 (1900); W. Murray Morrison, *Journal Institution of Electrical Engineers*, vol. 31, p. 400 (1901); Haber, *Zeitschr. für angew. Chemie*, p. 215 (1901); Richards, Electro-Chemical *Industry*, vol. 1, p. 49 (1902).

carbon-lined cast-iron pots 6 feet long × 3 feet wide × 10 inches deep; these form the cathode in which the metal is collected. The anodes are some 40 in number, and each carries about 250 amperes, the E.M.F. being 5 volts; thus some 65 H.P. are absorbed in each cell, a number of the cells being connected in series.

So far as the actual electrolysis is concerned, the recent scientific work of Haber [1] is of interest; he has been able to explain the conditions necessary for satisfactory working. One of the most important factors in the expense of manufacture is the cost of purifying the bauxite, which is an hydrated oxide of aluminium, containing always a considerable percentage of silica, iron oxide, and titanic acid. The process most generally in use at the present time is that of Bayer, which is purely chemical in nature.

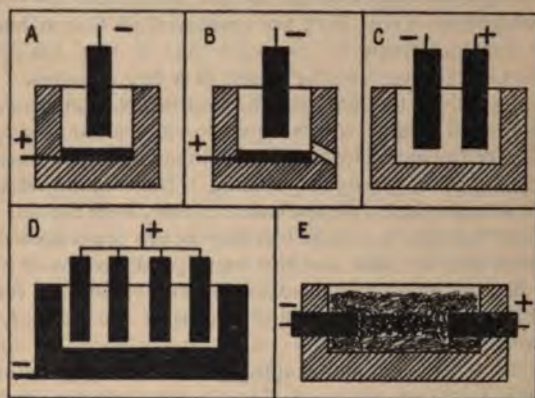

FIG. 13.—Principal Types of Electric Furnaces.

A. Represents the ordinary pot furnace in which the current passes between a movable carbon electrode, and a carbon or other plate forming the base of the furnace.

B. Is an example of a tapping furnace, being otherwise very similar to A.

C. In this case the furnace proper is formed of insulating material (generally lined with the unacted-on mixture), the current passing between the two carbons.

D. Represents electrolytic furnaces such as are used for aluminium, zinc, &c., the current passes from one or several carbon electrodes forming the positive electrode to the negative pole, which is formed by the lining of the furnace.

E. Resistance furnace: most frequently provided with a core through which the current is passed, and thus the surrounding material is heated.

All the types except D are suitable either for alternating or direct current, the former being most frequently in use.

A method recently invented by Hall proposes the purification of the bauxite in an electric arc furnace of type A, Fig. 13, by heating the mineral in contact with sufficient carbon or other reducing material to remove the impurities, and leave the oxide in a fused state. The oxide after cooling is removed, and is a grey friable material easily soluble in the cryolite; it is considered that this may considerably cheapen the manufacture. The total output of aluminium can be gleaned only from very insufficient data, the companies refusing to give

[1] *Haber and Geipert, Zeitschr. für* Electrochemie, vol. 8, pp. 1, 26, 607 (1902).

official statistics; however, it is said to have been approximately 7,500 tons for 1901. The yield usually obtained is about 1 lb. of aluminium per 12 H.P. hours.

SODIUM.—This metal has for long been produced by the Castner process,[1] which is carried out at Runcorn, Niagara, in Germany and France, and consists in electrolysing fused caustic soda; the metal being lighter than the electrolyte rises to the top and is removed from time to time. The construction of the cell is of some interest, since by an ingenious arrangement of the anode and cathode a high current efficiency is obtained. The important consideration seems to be the exact regulation of the temperature, since at already 20° above the melting point the recombination of the separating sodium is said to be so active that no metal comes to the surface. Several descriptions of the technical process have been given, and the current efficiency stated to be 70–90 per cent. At Niagara some 120 cells, each taking 1,200 amperes at 5 volts, are in use.

A careful study of this electrolysis has recently been made by Le Blanc and Brode,[2] who prove that the primary products are sodium at the cathode and oxygen and water at the anode; they do not seem, however, to have carried out many experiments at the lower temperatures, but state that the water remaining in the fused substance reacts with the sodium produced at the cathode, thus lowering the efficiency to 50 per cent., which is considerably below that quoted for the technical process. Sodium has also been produced by the Darling[3] process, in which the fused nitrate is electrolysed in cast-iron vessels provided with a thick diaphragm of magnesia and cement, with the production of nitric acid at the anode, and metallic sodium at the cathode. Each cell takes 400 amperes, but the voltage is as high as 15 volts.

CAUSTIC SODA.—The problem of the electrolytic production of caustic alkalies and chlorine has for long been an important one, and has already met with a large amount of success, the Griesheim-Elektron, Castner-Kellner, and Hargreaves-Bird processes being amongst those which are worked on a large scale; all these, however, deal with aqueous solutions, and we are therefore not concerned with them here. The direct treatment of fused salt offers considerable theoretical advantage, and several processes have been proposed, and amongst these we may mention the methods of Vautin, Hulin, and Acker,[4] all of which employ fused lead as a cathode to the sodium. The latter has been working since December, 1900, at Niagara Falls, with very satisfactory results, using some 3,250 H.P. The entire success depends on rapidly removing the sodium alloy as formed, since its diffusion into

[1] *English Patent* No. 13,356 (1890); *Electro-Chemical Industry*, vol. 1, p. 15 (1902); *Journal Soc. Chem. Industry*, p. 777 (1891); Rathenau and Suter, *German Patent* No. 96,672 (1896); R. Pauli, *Chemische Zeitschrift*, vol. 1, p. 497 (1902).

[2] Le Blanc and Brode, *Zeitschr. für Elektrochemie*, vol. 8, pp. 697–707, 717–729 (1902).

[3] Darling, *Journ. Franklin Instit.*, vol. 153, pp. 65–67 (1902).

[4] Vautin, *Eng. Patent* 13,568, 1893; *Journ. Soc. Chem. Industry*, vol. 13, p. 448 (1894). Townsend, *Electrical World and Eng.*, vol. 39, pp. 585–587 (1902). Acker, *Trans. Amer. Electro-chemical Soc.*, vol. 1, p. 165 (1902).

the mass of lead takes place only very slowly, and the richer alloys are unstable in presence of the fused salt. Acker has produced this rapid circulation by employing a steam injector, which causes the lead to flow rapidly past the anodes, and at the same time oxidises out the sodium, producing directly anhydrous fused NaOH. The temperature at which the steam and lead alloy come in contact is already high, but is still further raised by the heat of combination of the sodium, and thus of course any excess of steam passes off without combining with the caustic soda. The circulation is so good that the lead alloy in the cell does not average above 4 per cent. Na. The chlorine is drawn

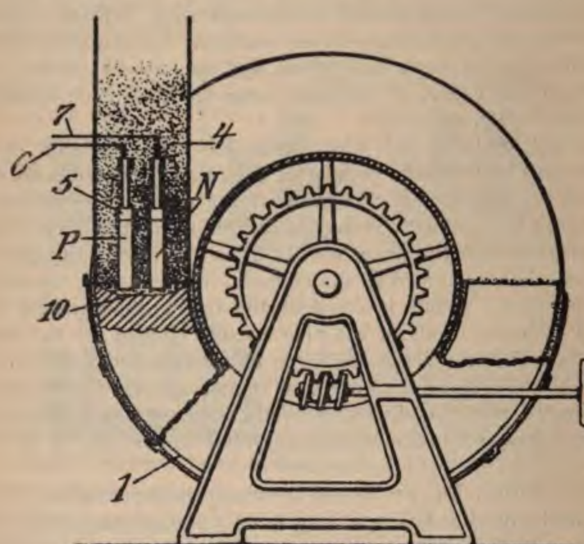

FIG. 14.—The Horry Calcium Carbide Furnace.

This is essentially a furnace of type C, Fig. 13, the current passing between two vertical carbons P and N, which are fixed. On the other hand, the enclosure containing the material is automatically lowered so soon as the mixture in the neighbourhood of the electrodes has been transformed into molten carbide. This is effected by constructing the furnace in the form of a drum which rotates very slowly, heating is produced in a space formed by the wide flanges of the drum, the drum itself, and plates bolted on to the periphery; these metal parts are sufficiently protected by the unacted-on material. Diametrically opposite the heating zone, at the back of the furnace the plates are unbolted and the carbide removed. This furnace combines several of the advantages of the continuous and discontinuous types; it is continuous in the true sense of the word since the power is never turned off, on the other hand the molten carbide is not removed until it has cooled down and imparted a considerable amount of its heat to the surrounding material. The speed of rotation of the wheel is regulated in such a manner as to keep the current approximately constant.

out by a fan, and used for producing bleaching powder. The cells, which are arranged in pairs, one on each side of a central flue, are cast-iron tanks with linings above the level of the fused lead.

Here again the inherent difficulties of furnace-linings are surmounted *by leaving* a sufficient coating of unfused material which protects the

walls. A central channel is provided below the actual electrolytic vat, by which the lead which has been freed from its sodium is returned.

The distance between the fused lead cathode and the carbon anode is very small, and thus the internal resistance is kept low. The anodes in each cell are four in number, and are formed of graphite $7\frac{1}{2} \times 14$ inches; they carry 2,000 amperes each, the voltage being 7. Forty-five cells are run in series. The anhydrous caustic formed runs over an iron lip into a receptacle placed to receive it. The current efficiency averages 94 per cent.

ZINC.—Various processes have been proposed, and some are in actual use. The recently perfected "Phœnix" process of Swinburne and Ashcroft enables zinc to be produced by the electrolysis of the fused chloride, and is of particular interest on account of its application to the treatment of complex Broken Hill ores.[1]

MANGANESE.[2]—In some respects very similar to the aluminium process is that recently worked out by Simon and Gin for the production of manganese. A bath of fused calcium fluoride is employed in which the oxide is dissolved and submitted to electrolysis, carbon being added to assist in the reduction. The advantage of complete regulation of the temperature is in this case of great importance, since manganese is already easily volatile at temperatures only slightly above its melting-point.

PHOSPHORUS.[3]—Already in 1888 Readman and Parker perfected a satisfactory commercial process for the manufacture of phosphorus in the electric furnace. This method was tried on a large scale at the works of the Electric Construction Company at Wolverhampton, and seems since then to have been used by Messrs. Albright and Wilson at Oldbury. This firm is also closely connected with the Oldbury Chemical Company, who operate a plant at Niagara Falls. The method employed is very simple, the phosphate-mineral in a finely powdered state being mixed with carbon and sand, and heated in a closed electric furnace, the phosphorus distilling off and being collected under water. Other electric furnace methods are in use at Griesheim and in France, and there is no doubt that for long the advantages of the direct-furnace treatment have been fully made use of, although few details have been allowed to escape.

NITRIC ACID.[4]—The fascination of the direct synthesis of an important chemical compound has for long directed attention to the production of nitric acid from the nitrogen and oxygen of the air. The research of Rayleigh and Ramsey on the isolation of argon, followed by the im-

[1] Ashcroft, *Inst. Mining and Metallurgy*, 1901; *Electro-Chemist and Metallurgist*, pp. 244–249, 269–271 (1901).

[2] Simon, *Engl. Patent* No. 17,190 of 1900; Gin, *La fabrication électrique du Ferro-Manganèse en France, procédé Simon*, Paris, 1901.

[3] Readman, *Engl. Patent* 14,962 of 1888; Parker and Robinson, *Engl. Patent* 17,719 of 1888; Readman, *Journ. Soc. Chem. Industry*, vol. 10, p. 445 (1891); *Thorpe's Dict. of Chemistry*, vol. 3, p. 192: Machalske, *Electrical World and Engineer*, vol. 37, p. 360 (1901); Irvine, *Electrical World and Engineer*, vol. 38, p. 374 (1902).

[4] McDougall and Howles, *Manch. Lit. and Phil.*, vol. 44, part 4, No. 13, pp. 1–19 (1900); Bradley, *Electrical World and Engineer*, vol. 40, p. 159 (1902); Rayleigh, *Journ. Chemical Society*, vol. 71, p. 181 (1897).

portant British Association address delivered by Crookes in 1898, helped to emphasise the importance of this subject. The careful work carried out by McDougall and Howles, which has not received the attention it deserves, was more particularly directed to a study of the efficiency of this process. By employing an alternating high-tension arc in air they succeeded, by a study of the necessary conditions, in obtaining a yield of 300 gms. HNO_3 per 12 H.P. hours, in this way combining 51 per cent. of the air passed through their apparatus,

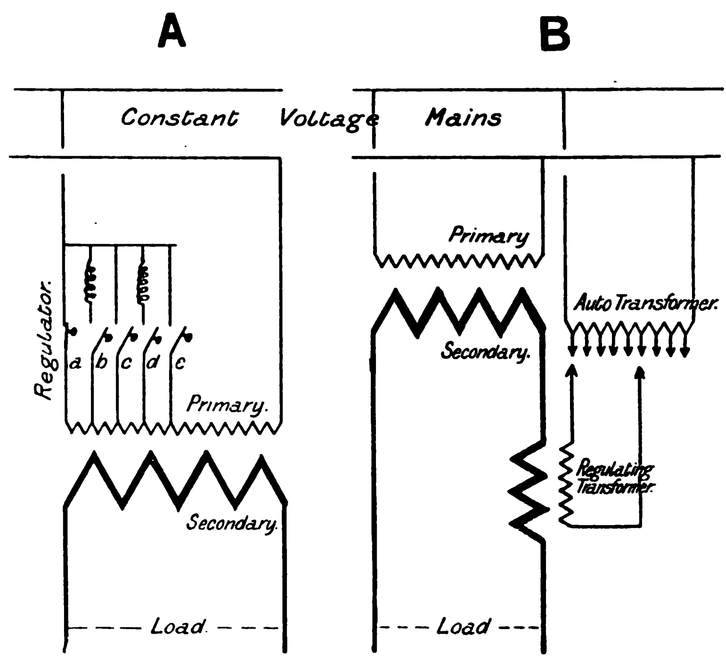

FIG. 15.—Regulating Transformers for Electro-chemical Work.

A. Shows the first method which consists in altering the transforming ratio by cutting out a certain number of the primary turns. As connected the E.M.F. of the secondary circuit would be a maximum, to reduce it the switch *B* is closed and *A* is opened, *C* closed and *B* in turn opened, each successive operation diminishing the E.M.F. by a fixed amount, the regulation being thus carried out without breaking the current.

B. Represents a second method which consists in using a boosting transformer. The current of the primary of the booster is adjusted according to the volts required in the secondary. In the diagram this is achieved by connecting it with a certain number of turns of an auto-transformer placed across the mains.

whilst with a mixture of two volumes oxygen to one volume of nitrogen the yield rose to 590 gms. per 12 H.P. hours. In most of their experiments they used a transformer giving 8,000 volts. The work of Bradley and Lovejoy at Niagara has given more favourable results from an economical point of view. A considerable amount of preliminary work pointed out the advantage of the direct current, and the apparatus *now working* employs a 10,000-volt continuous-current dynamo. As

will be seen in Fig. 17, the negative pole of the dynamo is connected to an axis carrying six radial arms, the positive poles being placed round the periphery of an iron cylinder which forms the combustion chamber. A choking coil is placed in each circuit. The actual apparatus comprises twenty-three such stars fixed one above the other on the same vertical axis, which revolves at the rate of 500 revolutions per minute, forming and breaking 414,000 arcs per minute. The chief function of this rotation is the rapid cooling down of the products of the combustion, which, if allowed to remain under the heating influence of the arc, would dissociate. For the same reason a rapid flow of air has been adopted, so that the issuing gases only contain about 2 to 3 per cent. oxides of nitrogen. The yield obtained is 1 lb. nitric acid per 7 H.P. hours. The process is considered to have already passed the experimental stage, and at the present time steps are being taken to start it on a commercial scale.

FUSED ALUMINA.—We have already mentioned the method of Hall for purifying bauxite by fusion in the electric furnace in presence of carbon or other reducing material. Our present consideration, however, is the manufacture of an artificial abraisive by the direct treatment of bauxite. The only method, so far as is known, in actual operation is that patented by Jacobs which is now being employed at Niagara Falls, by the Norton Emery Wheel Company, for long known as important manufacturers of abraisive articles of natural corundum. It has been found that the electric furnace product possesses advantages over the best grades of natural material. The bauxite is first thoroughly calcined in ordinary furnaces, and is then heated to fusion in an arc furnace of type B, Fig. 13. The plant at present in use, which is about to be further extended, employs some 500 H.P. and produces daily from 4–5 tons of fused alumina, called "alundum" to distinguish it from corundum. The material exhibits at times considerable crystalline formations.[1]

BARYTA.—This material is being prepared at Niagara by the United Barium Company, Barytes ($BaSO_4$), together with some reducing material, being treated in the electric furnace. The reaction which first takes place is as follows :—

$$4BaSO_4 + 4C = BaS + 3BaSO_4 + 4CO.$$

The Barium sulphide then reacting with the sulphate to give anhydrous baryta.

$$BaS + 3BaSO_4 = 4BaO + 4SO_2$$

In practice 500 k.w. tapping furnaces are in use, which yield a mixture of oxide and sulphide. These are easily separated in aqueous solution yielding a very good quality barium hydrate. The sulphide can afterwards be carbonated or otherwise worked up.[2]

CARBON BISULPHIDE.—As we have pointed out previously, the electric

[1] Jacobs, *U. S. Patent*; Gintl, *Zeitschr. für angew. Chemie*, p. 1173 (1901); Hasslacher, *German Patent* 85,021 of 1896.

[2] Jacobs, *Journ. Soc. Chem. Ind.*, vol. 21, p. 391; Limb, *Eng. Patent* No. 7,282 (1899).

furnace is usually employed for producing chemical reactions which require temperatures otherwise unattainable. In the manufacture of carbon bisulphide the advantage of electric heating lies, however, in the fact that a more perfect control of the furnace temperature can be thus attained, the temperature required being very low. The method invented by E. R. Taylor for producing this substance consists in the direct treatment in the electric furnace of charcoal and sulphur. The manufacture has been carried out for some time at Penn Yan, New York, the daily output being some 10,000 lbs. The furnaces are decidedly the largest at present in use in any electro-chemical works, being some 40 feet high by 16 feet diameter. The sulphur is fed in continuously so as to rise from below the electrodes where it comes in contact with coke, which forms a resistance bridge between the carbons, in this way becoming vapourised and brought into contact with the charcoal which fills the rest of the tower; combination takes

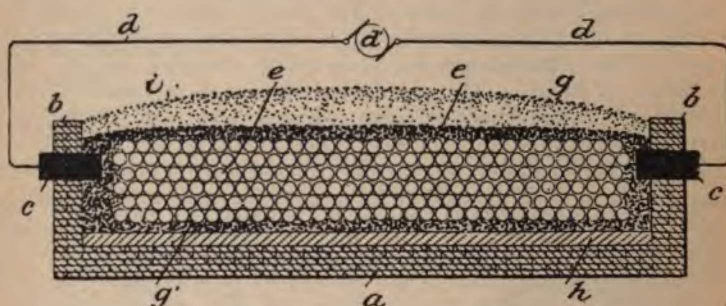

FIG. 16.—Acheson Graphite Furnace.

This shows the furnace fitted for graphitising electrodes; as will be seen, the current passes through the charge, which itself forms the heating resistance. The carborundum furnaces are of a similar type, but on account of the high resistance of the material are considerably shorter, and are provided with a central core of coke. Each furnace absorbs 1,000 H.P. and is run for twenty-four hours. It is capable of graphitising 3½ tons of carbon electrodes or 6 tons of anthracite.

place, and the CS_2 produced is led off and condensed. By the application of this process a very considerable cheapening in the cost of production has been effected.

STEEL.—The application of the electric furnace to the melting of steel, which was one of the first problems worked upon the Siemens, has been revived during the last few years in many different forms. The proposals, which not only include the manufacture of steel from pig-iron, but also the direct production by electric smelting of the ore have led to the design of many special types of furnaces.

De Laval[1] invented an ingenious furnace for making steel in which the metal was melted by bringing it in contact with fused oxide of iron heated electrically by resistance. This method is reported to have been tried on a large scale at Trollhättan in Sweden, but to have been a financial failure.

[1] *Jahrbuch der Elektrochemie*, vol. 1, p. 123 (1894).

Stassano's[1] furnace was in design somewhat similar to the blast furnace, the necessary heat being produced by the electric arc. A company was formed to work this process in Italy, but is said to have since ceased operations. One ton of metal was produced per 3,000 H.P. hours.

Harmet,[2] of the "Fonderies, Forges et Acieries," at St. Etienne, has worked out a method for treating iron ore in three stages, the carbon monoxide evolved by the reduction being partly used to heat the raw materials, whilst the reduced metal is transformed into steel in a separate electric furnace. Works are being erected at the present time for making use of this process.

Ruthenberg[3] has worked more particularly on the magnetic concentration of low grade ores and their subsequent fritting in the electric furnace.

Conley[4] proposes the reduction of iron ores by passing them between two high resistance plates which are kept heated by the passage of a current; the metal falls into a hearth, which is also electrically heated.

Gin has brought forward a method of heating mixtures of iron oxide and sulphide ores, with production of sulphuric acid and ferro-silicon as by-products.

Keller[5] has designed a furnace which is being employed in works at Kerrousse (Morbihan).

Processes of electric melting by induced currents have been proposed by Benedicks[6] in Sweden and Schneider[7] in France; in both cases the metal contained in an annular crucible forms the electric conductor surrounding an iron ring, in which rapid alterations in magnetic flux are produced. Benedicks' contrivance is similar in principle to a welding transformer, the entire secondary circuit of which is formed by the molten metal. This process is in actual work at Gysinge, where 300 H.P. are employed, and 1,500 tons steel can be produced per annum. Schneider, on the other hand, produces an alternating magnetic field by the rotation of a shuttle wound armature. The outlook of these inventions can only be regarded as immediately hopeful in localities in which water power is available at an exceptionally low cost; the value of any economically successful process, however, can hardly be over-estimated.

GLASS.—The accuracy with which temperatures produced by electric methods can be adjusted has led to several proposals for the application of the electric furnace to the glass industry, where, as is known,

[1] *Jahrbuch der Elektrochemie*, vol. 6, p. 320 (1899); *Zeitschr. für Elektrochemie*, vol. 8, pp. 61, 852 (1902); *Journ. Soc. Chem. Industry*, vol. 20, p. 816 (1902).

[2] Harmet, *Étude sur l'Electrometallurgie du Fer*, I. and II. Cf. also *Electrochemist and Metallurgist*, vol. 9, p. 18 (1902); *Zeitschr. für Eletrochemie*, vol. 8, p. 852 (1902).

[3] Ruthenberg, *Eng. Patent* 13,867, 1902; *Electrochemist and Metallurgist*, vol. 2, p. 12 (1902); *Jahrb. der Elektrochemie*, vol. 1, p. 516 (1902).

[4] Conley, *Electrochemist and Metallurgist*, vol. 2, p. 16 (1902).

[5] Bertholus, *Notice sur la Fabrication des aciers au Four Électrique*, Paris, 1902.

[6] Benedicks, *Eng. Patent* No. 18,921 of 1900

[7] Schneider, *Eng. Patent* No. 7,338 of 1901.

the temperature regulation is one of the most important factors. Trials are being made on a commercial scale at Plettenberg (Westphalia).[1] With regard to quartz, since the temperature required is very high, the electric is the only method available for fusing this material in bulk. Considerable difficulties will have to be overcome, however, before this most valuable substance can be manufactured on a commercial scale.

CONCLUSION.—Although the above account is by no means exhaustive, we feel that we have already reached, if not exceeded, the limits of space which are usually allotted for a paper of this kind. In our brief review of the subject it has in most cases only been possible to describe summarily for each product one particular method of manufacture, which, however, we have tried to choose as being one worked on a large commercial scale, but it must be borne in mind that by so doing

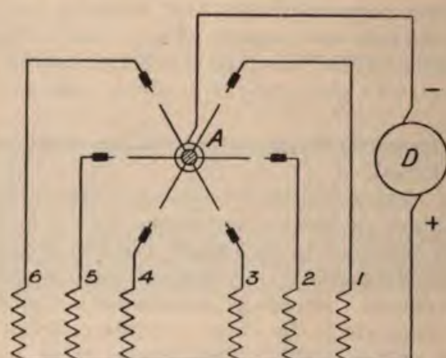

FIG. 17.—Diagram of Bradley Nitric Acid Plant Connections.

The dynamo D giving 10,000 volts has its negative pole connected with the revolving axis A, whilst the positive is connected to all the separate stationary electrodes through the choking coils 1, 2, 3, &c.

Twenty-three similar sets of stars are superimposed on the same axis. The positive electrodes are vertically one above the other; the negative electrodes, however, are displaced about 2½ degrees, the arcs being thus drawn out in rapid succession.

it has not been our intention to in any way detract from the value of other methods, which, although less generally used, may frequently possess advantages over those we have mentioned.

Perhaps, however, before closing we may be allowed to say a few words with regard to the theoretical efficiency of electric furnaces, about which so much has of late been written. Calculations have been made referring to each of the manufactures, but the reliability of these varies largely from case to case. When dealing with the highest temperatures very few data are available with regard to the specific heat, latent heat, or heat of combination of the various materials employed. It was previously customary to assume a constant specific heat, but

[1] *Zeitschr. für Elektrochemie*, vol. 8, pp. 419–421 (1902); Völker, *Eng. Patent* No. 23,903 (1900).

more recently this has been shown to be untenable, and most of the modern calculations consider the specific heat as a linear function of the temperature, relying for their values for the higher temperatures on a very considerable extrapolation. The actual specific heats have been experimentally determined for a limited number of substances up to some 1,500° C., and it is hardly necessary to insist on how considerable an error can be caused by boldly applying these values to twice the absolute temperature. In addition it must be remembered that the second factor in the calculation, the actual temperature of a furnace, is in most cases practically unknown.

The temperature of volatilisation of carbon is usually taken as 3,400° C., but this is no measure of the temperature of the arc flame, which may be considerably higher. On the other hand, the average temperature of the furnace is always considerably below that of the volatilisation point of carbon.

The consequence of this rash way of dealing with the subject is that the estimates of the theoretical amount of heat required differ by nearly 100 per cent., as has been shown by Kershaw in the case of calcium carbide.[1]

With regard to those of the resistance furnaces in which the temperature is comparatively low, more accurate data can be obtained, and finally, in the electrolysis of fused salts, the calculations give a much more trustworthy result, depending as they do largely upon the electro-chemical equivalents, which are known with some accuracy.

The determination of the total loss of heat by radiation from an electric furnace provides, however, valuable information on one of the important factors in the efficiency, but the final criterion in all commercial work must necessarily be the number of tons yield per H.P. year. In describing the various methods it has been of great advantage to have means by which personal experience of the process could be obtained by direct experiment. For this we are indebted to the foresight of Professor Arthur Schuster, F.R.S., who, thanks to the munificence of Mr. Ivan Levinstein, was able to make ample provision for experimental electro-chemistry in the building in which we are now assembled.

[1] Kershaw, *Electrician*, vol. 46, pp. 164, 245, 267 (1900).

Institution of Electrical Engineers.

FORM OF MODEL GENERAL CONDITIONS

RECOMMENDED

FOR USE IN CONNECTION WITH CONTRACTS FOR PLANT, MAINS, AND APPARATUS FOR ELECTRICITY WORKS,

As drafted by a Committee appointed for the purpose, and presented to the Council for adoption as the Model General Conditions recommended by the Institution of Electrical Engineers.

A Committee was appointed by the Council on the 20th of December, 1900, and met first on January 7th, 1901 ; it was then arranged:—

(1) To send copies of the original Draft, for the purpose of eliciting criticisms and suggestions, to the Electrical Plant Manufacturers' Association, to the Cable Manufacturers' Association, and to the Municipal Electrical Association, as well as to certain engineers and manufacturers, and

(2) To consider such replies as should be received when examining the draft Conditions in detail.

At the request of the Committee, the three Associations above named, and also the Engineering Employers' Federation, appointed delegates to confer with the Committee, and at a subsequent Council Meeting these delegates were formally added to the Committee, which was much strengthened and greatly assisted by their co-operation.

The meetings of the Committee were well attended, and the draft General Conditions were considered individually in great detail, the views of all parties being carefully studied, and, as far as possible, harmonised. The General Conditions, with the emendations and additions made in the first survey, were re-examined in detail, and were then submitted to Counsel. They were then discussed at an Ordinary General Meeting of the Institution, and were then reconsidered by the Committee and again submitted to Counsel, and laid before the Council of the Institution. The form of General Conditions hereto subjoined is that agreed to.

FORM

OF

MODEL GENERAL CONDITIONS

FOR

ELECTRICITY WORKS CONTRACTS.

LIST OF GENERAL CONDITIONS.

DEFINITION OF TERMS—

1.—In construing these Conditions and the annexed Specification, the following words shall have the meanings herein assigned to them :—

Here fill the full of the chasers.

The "Purchasers" shall mean *

and shall include their legal personal representatives, successors and assigns.

The "Contractor" shall mean the Tenderer whose tender shall under these conditions be accepted by the Purchasers, and shall include his legal personal representatives and assigns.

Here fill he name he Engi- r.

The "Engineer" shall mean Mr. † , or other the Engineer for the time being, or from time to time duly authorised and appointed in writing by the Purchasers to superintend the construction and erection of the work or works the subject of the Contract.

"Work" or "Works" shall mean and include work to be done and plant and materials to be provided by the Contractor under the Contract, and where appropriate according to the context "Work" or "Works" as used in these Conditions shall include or denote plant and materials.

The "Contract" shall mean the agreement to be entered into between the Contractor and the Purchaser under Clause 10 of these Conditions, and shall include the General Conditions, Specification, Drawings, Form of Tender and Schedule of Prices.

The "Specification" shall mean the specification annexed to these General Conditions.

The "Site" shall mean the Site of the Electricity Works, situate in

[*Here give full description of locality.*]

and any other place in the said where work is to be executed under the Contract.

"Writing" shall mean any written, typed or printed statement under or over signature or seal as the case may be.

Words importing the singular shall also import the plural and *vice versâ*.

DRAWINGS ISSUED WITH SPECIFICATION—

2.—The Drawings issued with the Specification are enumerated under the different Sections to which they refer. They will be issued only to Tenderers under these Sections.

OUNDATIONS AND BUILDERS' WORK—

3.—Unless otherwise specified, the necessary Foundations and builders' work generally will be provided by the Purchasers.

SE OF CRANE—

4.—Each Contractor for plant to be erected in the Engine House will be permitted for the purposes of the Contract to use, free of charge, but at his own risk, and entirely under the directions of the Engineer, the -ton overhead crane in the Engine House, but each Contractor will be required to leave the same in as good a condition as he finds it, fair wear and tear excepted, and he shall not so use it as to hinder or interfere with the use thereof by the Purchasers or any other Contractor.

TE—

5.—Proper access will be provided by the Purchasers to the place where the work is to be executed.

There will be (*or* will not be) a Railway siding of feet gauge on to the site.‡

‡ Full information as to the Site and point or points of access thereto should be set forth by the Engineer in this Clause or in some other writing.

ENDERER'S SPECIFICATION—

6.—The Tenderer is required to fill in the details of his Tender in the spaces provided for the purpose at the end of each Section of the Specification. Such statement will be accepted in lieu of a *detailed specification*, but the tenderer is at liberty to add any

details that he may deem desirable, and in the event of his doing so shall print or type the same and annex the added matter to the Specification returned by him, but such additional details shall not be binding on the Purchasers unless they are approved by them and incorporated in the contract.

DRAWINGS TO ACCOMPANY TENDER—

7.—The Tenderer must submit with his Tender the drawings enumerated in the section for which he is tendering, drawn to as large a scale as is convenient.

Detailed drawings are not required to be submitted with the Tender; but if the Tenderer wishes to call special attention to any detail of construction, he may submit a drawing of the same with his Tender. All drawings and samples submitted by unsuccessful Tenderers shall be returned within fourteen days of the date of the adjudication of the Purchasers upon the Tenders.

Note.—It is advisable that the advertisement inviting Tenders should notify a suitable place where the General Conditions, Specification and Drawings may be inspected, and should give particulars of the class of plant and apparatus required under each section as will enable Contracting firms to decide, without obtaining the Specification, whether they are able to tender, and should also state the amount of the Deposit to be paid for the General Conditions, Specification and Drawings.

TENDERS—

8.—The copy of the Specification herewith supplied to each Tenderer must be filled up, returned intact, together with the General Conditions and Drawings, sealed and marked "Tender for Electricity Works," addressed to

and must be received by him before p.m. on

The Purchasers reserve to themselves the right of accepting a separate Tender or separate Tenders for any one or more of the Sections of the Specification, but not for part of a section.

The Purchasers do not bind themselves to accept the lowest or any Tender, nor will they be responsible for, or pay for, expenses or losses which may be incurred by any Tenderer in the preparation of his Tender, except as provided by Condition 10.

The sum deposited by the Tenderer on application for the Specification will be refunded to him within fourteen days of the date of the adjudication upon the Tenders, unless in any case the Purchasers on the advice of the Engineer shall determine that the Tender was not made in good faith, in which case the deposit shall be forfeited. Extra copies of the Specification and General Conditions may be supplied by the Engineer to Tenderers on payment of (*five shillings*) per set, and extra copies of the Drawings at a reasonable price. The Engineer shall decide whether a Tenderer should or should not be so supplied.

CONTRACTOR TO INFORM HIMSELF FULLY—

9.—If the Contractor shall have any doubt as to the meaning of any portion of these General Conditions or of the Specification

he shall, before signing the Contract, set forth the particulars thereof, and submit them to the Engineer in writing, in order that such doubt may be removed.

CONTRACT AND BOND—

10.—The Contractor shall enter into a sealed Agreement for the proper fulfilment of the Contract, and provide two Sureties, or Grantors of an Insurance or Guarantee Policy, whose names shall be set out in the Tender, and be subject to the approval of the Purchasers, and who shall execute a joint and several bond or grant an Insurance or Guarantee Policy to the extent of 10 per cent. of the value of the Contract, by way of suretyship for the due and faithful performance of the Contract, as defined by these conditions, such suretyship to be binding notwithstanding any variations, alterations, directions, or extensions of time to be made, given, conceded, or agreed under these conditions.

The required agreement and instrument of suretyship shall be prepared or approved by or for the Purchasers and they shall forward the same to the Contractor not less than thirty days from the date of acceptance of his Tender.

In case the Contract and Bond or Security shall not be executed by the Contractor and his Sureties, Insurers, or Guarantors respectively within thirty days after the same shall have been presented to the Contractor for that purpose, the Purchasers shall not, unless they think fit, be bound by their acceptance of the Tender, or by the Contract, but the same shall, at the option of the Purchasers, be absolutely void, and if the Purchasers, by notice in writing to the intending Contractor, declare the same to be void, the Purchasers shall not be liable to or for any claim or demand from the Contractor, in respect of work then already done or materials furnished, or in respect of any other matter or thing whatsoever.

In case the Contract shall not be executed by the Purchasers within thirty days after receiving the executed portion from the Contractor, the Tenderer shall not, unless he thinks fit, be bound by his Tender, but the same shall, at his option, be absolutely void, and if the Tenderer, having duly complied with these conditions, shall, by notice in writing to the Purchasers, declare the same to be void, the Tenderer shall not be liable to or for any claim or demand from the Purchasers, but the accepted Tenderer shall be entitled to be repaid the proper expenses of his Tender, together with any sum or sums in respect of work then already done or materials furnished, at the request in writing of the Purchasers.

The expenses of completing and stamping the Contract and Bond or Insurance or Guarantee Policy shall be paid by the Purchasers, and the Contractor shall be furnished with an executed counterpart of the Contract.

CONTRACT DRAWINGS—

11.—The Contractor shall submit, for the Engineer's approval, a preliminary set of the drawings set out under each Section of the Specification, by the dates therein indicated.

Within fourteen days of the receipt of the preliminary set of drawings the Engineer must signify his approval or otherwise of the same.

Within fourteen days of the notification by the Engineer to the Contractor of his approval of the preliminary set of drawings, two additional sets, in ink on tracing cloth or ferrogallic prints mounted on cloth, of the drawings as approved shall be supplied to him by the Contractor and be signed by him and by the Contractor respectively and be thereafter deemed to be the "Contract Drawings."

These drawings when so signed shall become the property of the Purchasers and be deposited with the Engineer, and shall not be departed from in any way whatsover except by the written order of the Engineer as hereinafter provided. During the execution of the works one of the sets of drawings shall be available for reference on the site.

In the event of the Contractor desiring to possess a signed set of drawings, he may submit three sets, and in this case the Engineer will sign the third set and return the same to the Contractor.

The Contractor shall supply from time to time such additional drawings of any details as the Engineer may deem necessary for the execution of the work, but the Contractor shall not be called upon to furnish drawings of constructional details further than those which in the opinion of the Engineer are required for the purposes of the Contract.

The Engineer shall have the right, at all reasonable times, to inspect, at the works of the Contractor, drawings of any portion of the work.

If the Contractor shall not submit the drawings within the time specified, or subsequently within seven days after the Purchasers or the Engineer shall in writing have required him so to do, and if the delay shall not have been occasioned by the Purchasers or the Engineer, or any other Contractor, or other reasonable cause, the Purchasers may notify in writing to the Contractor that they will not be bound by the Contract, and on such notification the Contract shall be avoided and the Purchasers shall not be liable to or for any claim or demand from the Contractor in respect of work then already done or material furnished, or in respect of any other matter or thing whatever.

Or, *alternatively*, the Purchasers may, at their option, maintain the Contract, and in such case the Contractor shall pay or allow to them in account all expenses incurred by such default.

DRAWINGS OF FOUNDATIONS—

12.—Where Foundations are necessary, the Contractor shall supply the Engineer with Drawings of the Foundations necessary for his Plant, such Foundations being provided by the Purchasers (see Clause 3). The Contractor shall insure, by means of templates, the correct position of the foundation bolts and other details.

DEFECTS IN CONTRACTOR'S DRAWINGS—

13.—The Contractor shall be responsible for any mistake that may arise from any defect in the drawings supplied by him, and for any costs, damages, or expenses which may be sustained or incurred by the Purchasers by or in consequence of any such mistake, unless such drawings shall have been approved and signed by the Engineer.

DRAWINGS OF COMPLETED WORKS—

14.—Within one month of the taking over of the plant under Clause 41, and of the receipt of a list from the Engineer of working drawings of such portions of the plant as may reasonably be required for the future use of those in charge of the Works, the Contractor shall supply the same at the cost of production.

SUB-LETTING OF CONTRACT—

15.—The Contractor shall not, without the consent in writing of the Engineer, assign his Contract, or any substantial part thereof, nor under-let the same or any substantial part thereof, nor make any sub-contract with any person or persons for the execution of any portion of the works other than for raw materials, for minor details, or for any part of the work of which the makers are named in the Contract.

APPROVED APPARATUS—

16.—In all cases where plant or apparatus of "approved" type or make is required by the terms of the specification, the Engineer's approval thereof in writing must be obtained before such plant or apparatus is constructed or ordered, provided that if the Contractor shall have submitted with his tender drawings of the apparatus which he has included in his tender or otherwise shall have described the same in detail, the Engineer shall not

have the right to demand other plant or apparatus of a greater value than that so drawn or described.

NOTICES—

17.—All notices to the Contractor for the purposes of the Contract and these General Conditions, shall be sufficiently authenticated if signed by the Purchasers or by the Engineer; all notices from the Purchasers to the Contractor, and from the Contractor to the Purchasers shall be served respectively upon them personally, or by letter addressed to the places of business respectively named in the Contract, and any notice by letter shall be deemed to have been duly served at the time when the letter containing the same would be delivered in the ordinary course of post, and in proving such service it shall be sufficient to prove that the notice was properly addressed and posted. Provided always that if the Contractors or the purchasers respectively shall, after the Contract shall have been entered into, change his or their place of business and shall notify such change to the other of them in writing, all future notices if sent by letter shall after the receipt of such notification be addressed to such new place of business.

PATENT RIGHTS—

18.—The Contractor shall fully indemnify the Purchasers against any action, claim or demand, costs, or expenses arising from or incurred by reason of any infringement or alleged infringement of letters patent, trade mark or name, copyright or other protected rights, in respect of any plant, work, material or thing, system or method of using, fixing, working or arrangement used or fixed or supplied by the Contractor, but such indemnification shall not be operative in respect of any system or method of use that may be specifically mentioned by the Specification. All payments and royalties payable in one sum or by instalments or otherwise shall be included by the Contractor in the prices named in his Tender, and shall be paid by him to those to whom they may be due or payable.

In the event of any claim being made or action brought against the Purchasers in respect of any such matters as aforesaid, the Contractor shall be immediately notified thereof, and he shall, with the assistance, if necessary, of the Purchasers, but at his sole expense, conduct all negotiations for the settlement of the same, or any litigation that may arise therefrom.

MANNER OF EXECUTION, QUALITY OF MATERIAL, ETC.—

19.—The plant is to be manufactured, constructed, provided, erected in position, and maintained in accordance with the Contract, in the best and most substantial and workmanlike manner, and, unless otherwise specified, with materials of the best and most approved qualities for their respective uses.

POWER TO VARY OR OMIT WORK—

20.—The Contractor shall not alter, in any way whatsoever, any of the work, except as directed in writing by the Engineer; but the Engineer shall have full power from time to time during the execution of the Contract to alter, amend, omit, or otherwise vary any of the work, without in any way affecting or vitiating the Contract, and the Contractor shall carry out such alterations, amendments, omissions, variations, or directions, and be bound by the same conditions, as far as applicable, as though the said alterations, amendments, omissions, variations, or directions occurred in the Contract. The difference of cost, if any, occasioned by any such alterations, amendments, omissions, variations, or directions, shall be added to or deducted from the Contract Price as the case may require. The amount of such difference, if any, shall be ascertained and determined in accordance with the rates specified in the Schedules of Prices, so far as the same may be applicable, and where the rates are not contained in the said Schedules, or are not applicable, they shall be settled by the Engineer and Contractor jointly. But the Purchasers shall not become liable for the payment of any charge in respect of any such alterations, amendments, variations, or directions unless the instruction for the performance of the same shall have been given in writing by the Engineer, nor unless such instruction shall state that the matter thereof is to be the subject of an extra or varied charge, nor unless the particulars of his claim shall be set forth in writing by the Contractor, and furnished to the Purchasers within thirty days after the execution of the same; but subject to these conditions being duly complied with, the Purchasers shall be bound by such particulars unless they or the Engineer object thereto in writing within thirty days after delivery thereof.

In the event of the Engineer requiring to dispense with or add to any part of the plant or works to be done under this Contract such reasonable and proper notice shall be given to the Contractors as will enable them to make their arrangements accordingly.

Unless the Contractor shall otherwise agree in writing, the total sum of money set out in the Contract shall not be affected by such alterations, amendments, omissions, variations, or directions to the extent of more than 10 per cent. of the amount of the Contract. Provided always that in cases where goods or materials are already prepared, or any matter or patterns made or work done that require to be altered in respect thereof, a reasonable sum shall be allowed by the Engineer.

EGLIGENCE—

21.—If the Contractor shall fail to execute the work with due *diligence and expedition*, or shall refuse or neglect to comply with

any orders given him in writing by the Engineer, or shall fail to execute any other matter stipulated in the Contract, or shall contravene the provisions of the Contract, the Purchasers shall, after seven days' notice to the Contractor, in writing, be at liberty to employ other workmen, and forthwith perform such work as the Contractor may have failed to do, or, if the Purchasers shall think fit, it shall be lawful for them to take the works wholly, or in part, out of the Contractor's hands and re-contract with any other person or persons, or provide any other materials, tools, tackle, or labour for the purpose of completing the works or any part thereof, and the Purchasers shall, without being responsible to the Contractor for fair wear and tear of the same, have the free use of all the materials, tools, tackle, or other things, the property of the Contractor, which may be on the site, for use at any time in connection with the work, to the exclusion of any right of the Contractor over the same.

If the cost of completing the works exceed the balance due to the Contractor, the said materials, tools, tackle, or other things may be sold by the Purchasers, and the proceeds applied towards the payment of such difference. Any outstanding balance existing after crediting the proceeds of such sale shall be paid by the Contractor on the certificate of the Engineer, but when all expenses, costs, and charges incurred in the completion of the work are paid by the Contractor, all such materials, tools, tackle, or other things shall be removed by the Contractor.

DEATH, BANKRUPTCY, ASSIGNMENT, AND SUB-CONTRACTING—

22.—The conditions and penalties in favour of the Purchasers contained in the last preceding condition may, subject as hereinafter provided, be enforced by the Purchasers if the Contractor die, go into liquidation, become bankrupt or insolvent, or have a receiving order made against him, or compound with his creditors, or propose any composition to his creditors for the settlement of his debts, or assign his Contract without the consent of the Purchasers, or if the Contract become vested in any other person, or if he commit any act of bankruptcy, or carry on his business under an Inspector or a Receiver for the benefit of his creditors, or permit any execution to be levied on his property, or if he sub-contract for any portion of the work otherwise than as provided in Clause 15. Provided that the consent of the Purchasers to an assignment of the Contract shall not be unreasonably withheld. In the case of the death, liquidation, insolvency, or other disability or act as aforesaid of the Contractor, his executors or other representatives in law of his estate shall have the option of carrying out the Contract subject to the Executors providing such additional

surety as may be required by the Purchasers as will bring the amount of the surety up to the Contract value of the work for the time being remaining unexecuted.

INSPECTION AND TESTING AT MAKER'S WORKS—

23.—The Engineer, and his duly authorised representative, shall have at all reasonable times access to the Contractor's Works, and shall have the power at all reasonable times to inspect, examine, and test the materials and workmanship of the plant during its manufacture there; and if part of the plant is being manufactured on other premises, the Contractor shall obtain for the Engineer and for his duly authorised representative permission to inspect as if the plant were manufactured on his own premises.

The Engineer shall, on giving fourteen days' notice in writing of his grounds of objection, have liberty to reject all or any materials, plant or workmanship, which in his opinion are not in accordance with the Contract, or are defective for any reason whatever, and such rejection shall be operative at the expiration of such notice, provided that, if notice of any such rejection, setting forth the reason for such rejection, be not sent to the Contractor within fourteen days after the grounds upon which such rejection is based have come to the knowledge of the Engineer, he shall not be entitled to reject the said materials, plant or workmanship on these grounds.

The Contractor shall give the Engineer not less than seven days' notice of any material being ready for testing, and unless otherwise arranged, the Engineer, or his representative, shall proceed to the Contractor's Works within three days of the date on which the material is notified as being ready; failing which visit the Contractor may proceed with the tests, and, in the absence of the Engineer, the tests shall be taken as if they were made in his presence.

TESTING APPARATUS—

24.—In all cases where the Contract provides for tests, whether at the works of the Contractor or the sub-contractor or on the site, the Contractor, except where otherwise specified, shall provide, free of charge, such labour, materials, chemicals, coal, oil, waste, apparatus, and instruments as the Engineer may consider requisite from time to time, and as may reasonably be demanded, to efficiently test the Plant, material or workmanship, in accordance with the Contract, and shall at all times give facilities to the Engineer or to his authorised representative to accomplish such testing

The apparatus, instruments, unused material, and apparatus so provided by the Contractor shall remain the property of the Contractor.

In the case of Cable contracts, current for tests on site shall be supplied free to the Contractors at the pressure of the ordinary supply.

DELIVERY OF MATERIALS—

25.—No Plant or materials shall be forwarded until an intimation in writing shall have been given to the Contractor by the Engineer that the Purchasers are ready to take delivery.

If the Purchasers withhold the forwarding of instructions so as to prevent the Contractor giving delivery by the dates stipulated in the Contract, the Purchasers shall bear the cost of the storage and protection, including fire insurance, of the Plant and materials, and make payments therefor as if delivery had been given, provided that possession thereof and the property therein be duly secured to the Purchasers.

ACCESS TO SITE—

26.—In the execution of the work, no persons other than the Contractor, or his duly appointed Superintendent, sub-Contractors and Workmen, shall be allowed to do work on the site, except by the special permission, in writing, of the Engineer, but access to the works at all times shall be accorded to the Engineer and his representatives, and other officials or representatives of the Purchasers.

MATERIALS BROUGHT ON TO THE SITE—

27.—The Contractor shall provide all materials, labour, haulage power, tools, tackle, and plant of every description, necessary to execute and complete the works in an efficient and satisfactory manner. All such materials, plant, tools, and tackle (except as provided by Clause 24 with regard to instruments and apparatus for testing and empty drums and packing cases), brought to and delivered upon the site for the purpose of the work, shall, from the time of their being so brought, vest in and be the property of the Purchasers until the completion of the Contract, when the property in any surplus materials, and in the plant and tools, shall revert to the Contractor, unless there shall be due, owing to, or accruing, or to accrue, from the Contractor to the Purchasers, any money or moneys under, or in respect of or by reason of this Contract, in which case the Purchasers shall be at liberty to sell and dispose of such surplus materials, plant and tools as they shall think fit, and to apply the proceeds in or towards the satisfaction of such money or moneys so due, owing or accruing, or to accrue to them as aforesaid.

If application be made to the Engineer by the Contractor, he may at any time permit the removal of such machinery, plant, tools, and tackle as may not be required for the execution of work under this contract, or which may be required by the Contractor for uses elsewhere.

ENGINEER'S SUPERVISION—

28.—All the works are to be carried out under the direction, control, and to the entire satisfaction in every respect of the Engineer; but the Contractor shall be responsible for the accuracy of his work, and no plea as to the acts, order, or general supervision of the Engineer otherwise than instructions given by him in writing will be admitted in justification of any errors of construction or fixing.

ENGINEER'S DECISIONS—

29.—In respect of all matters which are left to the decision or certificate of the Engineer, the Engineer shall, if required so to do by the Contractor, give in writing a decision thereon, and his reasons for such decision, or if he shall withhold any certificate then his reasons for so doing. All decisions of the Engineer shall be subject to the right of arbitration reserved by these conditions.

CONTRACTOR'S SUPERINTENDENT AND WORKMEN—

30.—The Contractor shall constantly employ at least one competent Superintendent to superintend the erection of the plant and the carrying out of the works. The said Superintendent shall be present on the site during working hours, and shall be prepared to receive from time to time orders and instructions from the Engineer or his duly authorised representative.

The said Superintendent, if objected to by the Engineer, on account of incapacity, misconduct, or negligence, shall be removed by the Contractor, and the Contractor shall, after receiving formal objection in writing, forthwith replace him by another Superintendent, competent to fulfil his duties.

The Engineer shall be at liberty to object to any person employed by the Contractor in the execution of or otherwise about the works who shall, in his opinion, misconduct himself or be incompetent or negligent, and the Contractor shall forthwith remove the person so objected to, and if necessary replace him by a satisfactory person who shall be the servant of and be remunerated by the Contractor.

LIABILITY FOR ACCIDENTS AND DAMAGE—

31.—The Contractor shall properly cover up and protect such of the work as may be liable to sustain injury by exposure. He shall also take every necessary, proper, timely and useful precaution against accident or injury to the plant, and shall be and remain answerable and liable for all losses, damages or injury which, during the progress of the work, and until it be taken over under Clause 41, may arise or be occasioned by the acts or omissions of the Contractor or his servants, but not for any subsequent consequential loss or damage, nor for any breakage or injury, wholly or partially caused by, or arising from, the acts of the Purchasers or others, or due to circumstances over which the Contractor has no control; and all such losses, damages, or injuries, if sustained by the Purchasers, shall be made good in the most complete and substantial manner by, and at the sole cost of the Contractor, and to the satisfaction of the Engineer, and the Contractor shall indemnify the Purchasers against all claims and demands in respect of such losses, damages, or injuries, if sustained by any other person or persons.

The Contractor shall likewise, until the Plant shall have been taken over under Clause 41, indemnify and save harmless the Purchasers against actions, suits, claims, demands, costs or expenses arising in connection with the Works under the Workmen's Compensation Act, 1897, and any other statute in force at the date of the Contract dealing with the question of the liability of employers for injuries sustained by employees.

In the event of any claim being made, or action brought against the Purchasers arising out of the matters referred to in this Clause, the Contractor shall be immediately notified thereof, and he shall, with the assistance if necessary of the Purchasers, but at his sole expense, conduct all negotiations for the settlement of the same, or any litigation that may arise therefrom. The Purchasers will, at the expense of the Contractor, afford all available assistance for any such purpose.

REPLACEMENT OF DEFECTIVE WORK OR MATERIALS—

32.—If during the progress of the work on site, the Engineer shall decide and notify in writing to the Contractor that the Contractor has executed any unsound or imperfect work, or has supplied any plant or materials of inferior quality to those specified, the Contractor shall at his own expense, within twenty-four hours of his receiving the notice, proceed to alter, re-construct, or remove such work, or supply fresh materials up to the standard of the Specification, and in case the Contractor shall fail to comply with such orders, the Purchasers may, without further notice, remove the work or materials complained of, and, at the cost of the Contractor, perform all such work or supply all such materials.

DEDUCTIONS FROM CONTRACT PRICE—

33.—All costs, damages, or expenses which the Purchasers may have paid, or be liable to pay, or which shall have become forfeited to the Purchasers as provided for by these Conditions and by the Specification, shall be paid by the Contractor to the Purchasers on the certificate of the Engineer, or if not so paid may be deducted from any moneys in their hands due or becoming due to the Contractor under the Contract, or recovered by action at law, or otherwise from the Contractor.

TERMS OF PAYMENT AND CERTIFICATES OF ENGINEER—

34.—The Contractor shall from time to time be entitled, upon the Certificates of the Engineer, to payments by the Purchasers by instalments in accordance with the following provisions :—

I.—As the Works progress, 80 per cent. upon the Contract value of the Work from time to time delivered or executed on the site to the satisfaction of the Engineer.

II.—The remaining 20 per cent. (referred to herein as retention money) in respect of each distinct Section or Part of a Section of the works as follows :—

(*a*) 10 per cent. at the expiration of one month after the Works shall have been taken over by the Purchasers under Clause 41 or alternatively, at the option of the Contractor, at the expiration of one month after the Works shall have been put into beneficial use by the Purchasers.

(*b*) 10 per cent. at the expiration of nine months after the first 10 per cent. becomes due under (*a*).

No part of the " Retention Money " will be payable at the time at which payment of the same ought otherwise to be made under the Contract, unless in the opinion of the Engineer the Works are then in good repair, and condition, and sound working order, fair wear and tear and accidental injury or damage by persons other than the Contractor's servants and not due to faulty workmanship or material, excepted. Provided, however, that where the defects are not of such importance as to affect the full beneficial use of the Works, the retention of the whole instalment shall not be insisted on, but the Purchasers shall be entitled to retain such less sum of money as, in the opinion of the Engineer, represents the damage to the Purchasers arising out of incomplete or defective details. Any sum retained under this clause will become due upon the adjustment of such details to the satisfaction of the Engineer.

Every application to the Engineer for a Certificate must be accompanied by a detailed claim (in duplicate) setting forth in the order of the Schedule of Prices, particulars of the work executed to the date of claim, and the Certificate shall be issued within 14 days of the application for same.

Not more than one Certificate shall be issued in any one month in respect of the same section.

The Engineer may by any Certificate make any correction or modification in any previous Certificate which shall have been issued by him, and payments shall be regulated and adjusted accordingly.

DUE DATES OF PAYMENTS—

35.—Payments shall be made by the Purchasers within thirty days from the date of each certificate of the Engineer.

In the event of the Purchasers failing to pay the Contractor any amount certified by the Engineer, within the specified period, and in accordance with the Contract the Contractor shall have the right, on giving 14 days' notice in writing to the Purchasers or the Engineer, to stop all operations, and the expenses incurred in resuming work shall be paid by the Purchasers to the Contractor as an extra over and above the amount payable under the Contract.

CERTIFICATES NOT TO AFFECT RIGHTS OF THE PURCHASERS OR CONTRACTOR—

36.—No certificate of the Engineer on account, nor any sum paid on account by the Purchasers, shall affect or prejudice the rights of the Purchasers against the Contractor, or relieve the Contractor of his obligations for the due performance of the Contract, or be interpreted as approval of the work done or of the materials supplied, and no certificate shall create liability in the Purchasers to pay for alterations, amendments, or variations not ordered in writing by the Engineer, or discharge the liabilities of the Contractor for the payment of damages, whether due, ascertained or certified, or not, or of any sum against the payment of which he is bound to indemnify the Purchasers, nor shall any such certificates affect or prejudice the rights of the Contractor against the Purchasers.

SUSPENSION OF WORKS—

37.—The Purchasers shall pay to the Contractor all reasonable expenses arising from suspension of Works by order in writing of the Purchasers or the Engineer unless such suspension be due to some default on the part of the Contractor.

DATES OF COMPLETION—

38.—The Works shall be completed on the site and ready for beneficial use or for testing by the date named under each Section, or by such other date (if any) as may be incorporated in the Contract.

Provided always that, if by reason of extra work, alterations in, or deviations from the Specification, directed in writing by the Engineer, or by reason of the suspension of the works under the direction of the Engineer, or of unusual inclemency of the weather, or by reason of civil commotion or general or local strikes, or lock-outs, or combinations of workmen, or in consequence of fire or of any unpreventable accident to or breakage of machinery in the manufacturers' premises or on the site, causing a delay in the supply of plant or materials to the Contractor, or by reason of the non-completion of a Section of the Contract executed by another Contractor, or by any act or default on the part of the Purchaser, or of other cause beyond the reasonable control of the Contractor, or by any delay on the part of the Purchaser to give forwarding instructions to the Contractor under Clause 25, the Contractor shall have been unduly delayed or impeded in the completion of the work, the Engineer shall, on the receipt of a written request from the Contractor, grant from time to time, and at any time or times, by writing under his hand, such extension of time, either prospectively or retrospectively, and assign such other day or days for the completion as to him may seem reasonable, without thereby prejudicing, or in any manner affecting, the validity of the Contract, and any and every such extension of time shall be deemed to be in full compensation and satisfaction for, and in respect of, any and every actual and probable loss sustained or which may be sustainable by the Contractor in the premises, and shall in like manner exonerate him from any claim or demand on the part of the Purchasers for, and in respect of, the delay occasioned by the cause or causes in respect of which any and every such extension of time shall have been made, but not further or otherwise, nor for, or in respect of, any delay continued beyond the time mentioned in such writing or writings respectively, provided that unless such request be made within two weeks after the expiry of the calendar month in which the delay existed no such extension of time shall be granted.

The Contractor shall not be called upon to commence any work which is of a nature requiring a building or structure for the reception or efficient installation thereof, and which building or structure is by the Contract to be provided by the Purchasers, unless and until such building or structure shall be in a condition sufficient for the reception or efficient installation of the Plant, and the Contract date of completion shall be extended *pari passu* with the delay in the providing of any such building or structure.

DAMAGES FOR DELAY IN COMPLETION—

39.—If the Contractor shall fail in the due performance of Contract by and at the time fixed under the Contract, whether by way of extension or otherwise, the Engineer shall, in writing certify the fact of such failure, and in such case the Contractor shall pay to the Purchasers, as and for agreed liquidated damages, the following amounts reckoned on the contract value of such portion only of the Works as cannot, in consequence of the delay, be used beneficially—

during the first four weeks between the appointed time and the actual time of completion, five shillings per £100 per week;

during the second four weeks, ten shillings per £100 per week;

during the third four weeks, fifteen shillings per £100 per week; and

during any subsequent week, twenty shillings per £100 per week.

PRELIMINARY TRIALS ON SITE—

40.—On the completion of the works on the site, the Contractor shall be at liberty, as far as convenient to the Purchasers, to make any preliminary trials that he may desire.

All expenses whatever of raising steam, or otherwise of or in connection with such preliminary trials, to which the Purchasers be put, shall be borne by the Contractor.

TESTS ON COMPLETION—

41.—On the completion of the works on the site, the Contractor, after giving the Engineer fourteen days' notice of his readiness to make the "tests on completion," shall test the operation thereof, either together or in sections, in the presence of the Engineer, and in all respects in accordance with and in manner provided by the Specification.

On the giving of such notice, the plant shall, for the purpose of the tests, be deemed to be complete, and no alterations or re-adjustments of the same shall be made within forty-eight hours before the time fixed for starting the tests, without the express permission of the Engineer in writing.

Should any alterations or re-adjustments be found necessary within forty-eight hours before the time fixed for starting the tests, the tests of the plant to which the alterations or readjustments are to be made may, at the sole option of the Engineer, be deferred for a period not exceeding fourteen days, and all reasonable expenses to which the Purchasers may be put by the deferring of the tests shall be borne by the Contractor.

The Contractor shall find and provide all necessary superintendence and labour for the purposes of the tests, and during the tests shall have the full working control of the plant.

If at the time agreed upon between the Contractor and the Engineer for the starting of the tests, the Engineer or his duly authorised representative shall fail to attend, the tests may proceed in his absence.

As soon as the tests have proved that the plant has completely fulfilled the Contract Conditions, the Engineer shall forthwith so certify in writing to both the Purchasers and the Contractor, and THEREUPON IT SHALL BE DEEMED THAT THE PURCHASERS HAVE TAKEN OVER THE PLANT.

If the works fail under the tests to fulfil the Contract Conditions, complete new tests shall, if required by the Engineer, or by the Contractor, be carried out upon the same terms and conditions, and upon payment to the Purchasers of all reasonable expenses to which they may be put by the repeated tests.

If the tests, proving that the works fulfil the Contract Conditions, be not made by the Contractor within one month after the date fixed under Clause 38 for the completion and the readiness of the works for beneficial use or for testing, and if, in the opinion of the Engineer, the tests are being unduly delayed, the Engineer may, in writing, call upon the Contractor under seven days' notice to make such tests, and on the expiry of such notice such tests shall forthwith be made by the Contractor.

If, after the expiry of the notice from the Engineer the Contractor neglects to make such tests, the Engineer may proceed to make such tests himself at the Contractor's risk and expense.

RIGHT OF USE—

42.—If the Contractor neglects to make the "tests on completion" by the dates stipulated under Clause 38, the Purchasers shall, nevertheless, have the right of using the works at their own expense for the supply of Electrical Energy or otherwise; but such use shall be at the Contractor's risk until he elects to make the "tests on completion" or until such tests prove that the plant fulfils the Contract Conditions. The Purchasers may, pending any Arbitration under the Contract, use any portion of the works reasonably capable of being used, but in such case the Contractor shall be entitled to be paid in respect of any work beneficially used, a sum equal to £5 per cent. per annum (according to the period of user) upon the amount withheld or deducted in respect of such work.

INTERFERENCE WITH TESTS—

43.—If any act of the Purchasers or of the Engineer, or the use of the work as above provided for, shall interfere with the Contractor carrying out the tests after the fourteen days' notice to be given by him to the Engineer, the payments to the Contractor shall be made as if final satisfactory "tests on completion" had taken place, but notwithstanding any such payments, the Contractor shall be liable to make, and shall make the said tests during the period provided for maintenance as and when required by the Engineer upon fourteen days' notice; and the obligations and liabilities of the Contractor shall be the same as if the tests had been made on the expiry of his fourteen days' notice.

The provisions of this condition as to payment shall apply in the event of such use, or any other act of the Purchasers, or of the Engineer, interfering with the remedying, by the Contractors, of any defects which may have appeared in the works.

REJECTION OF INEFFICIENT WORK—

44.—If the completed work or any portion thereof fails to pass the specified "tests on completion," or be defective in any way, the Engineer may reject such work or portion thereof, and the Purchasers shall then have the option of:—

(*a*) Permitting the Contractor to replace the defective work or,

(*b*) Themselves replacing the defective work by purchase from or contract with any other party or parties, or,

(*c*) Returning the defective Work and recovering the sum or sums paid or allowed on account of same.

In the event of (*a*), the substituted works shall be in all respects deemed to be subject to all the terms and conditions of the Contract.

In the event of (*b*), no further sum beyond that already paid to the Contractor in respect of the work in question shall be due or payable by the Purchasers to the Contractor in respect of such defective work, but the Contractor shall repay to the Purchasers any sum paid by them in respect of such work; and the Contractor shall also pay to the Purchasers any loss or damage to which they may be put by reason of the purchase or replacing of fresh work by them; it being agreed that if the Contractor shall fail to execute works in strict accordance with the Specification, it shall be lawful for the Purchasers, at their discretion, to obtain, without additional cost to them, the work in question from any other party or parties, or so to arrange for the execution of the works, as they may deem desirable, and that the Contractor

shall be liable for any loss suffered, or expenditure beyond the Contract prices incurred by the Purchasers in consequence of such failure.

In the event of (*c*), if the defective Work be required by the Purchasers for beneficial use, they shall be entitled to make use of the same for a reasonable time sufficient to enable them to obtain other work to replace it, the Contractors being allowed a reasonable sum for the use of the same.

MAINTENANCE—

45.—Until the final certificate shall have been issued the Contractor shall be responsible for any defects that may develop under normal and proper use arising from bad materials, design or workmanship in the Works. When called upon, in writing, by the Engineer to remedy such defects, the Contractor shall do so with due diligence, and unless such defects be remedied by the Contractor within a reasonable time, the Contractor shall be responsible for all losses and damages sustained by the Purchasers through such defects. If the defects be not remedied within a reasonable time, the Purchasers may proceed to do the work at the Contractor's risk and expense.

Until the final certificate shall have been issued, the Contractor shall have the right of entry by himself or his duly authorised representatives, at all reasonable working hours, upon all parts of the works for the purpose of inspecting the working and the records of the works and taking notes therefrom, and, if necessary, making any tests at reasonable times at his own risk and expense.

-QUIREMENTS OF LOCAL AUTHORITIES—

46.—The Contractor shall throughout the continuance of the Contract, and in respect of all matters arising in the performance thereof, promptly and effectually conform to all the requirements of any local or municipal authority in whose district the work may be executed, and provide for the safety and due convenience of the public.

RBITRATION—

47.—If at any time any question, dispute or difference shall arise between the Purchasers or their Engineer, and the Contractor, upon or in relation to or in connection with the Contract, either party may forthwith give to the other notice in writing of the existence of such question, dispute or difference, and such question, dispute or difference shall be referred to the Arbitration of a person to be mutually agreed upon, or, failing agreement, to some person appointed by the President for the time being of the *Institution of Electrical* Engineers.

Work under the Contract shall continue during the Arbitration proceedings.

The award of the Arbitrator shall be final and binding on the parties. Upon every or any such reference, the costs of and incidental to the reference and award respectively shall be in the discretion of the Arbitrator, who may determine the amount thereof, or direct the same to be taxed as between Solicitor and Client, or as between party and party, and shall direct by whom and to whom, and in what manner the same shall be borne and paid. This submission shall be deemed to be a submission to Arbitration, within the meaning of the Arbitration Act, 1889.

To be included where the work is to be done wholly or partly abroad or in Scotland.

CONSTRUCTION OF CONTRACT—

48.—The Contract shall in all respects be construed and operate as an English Contract and in conformity with English law, and all payments thereunder shall be made in [England and in] sterling money.

FORM OF TENDER.

SECTION .

....

To the ....

Gentlemen,

.... the undersigned, do hereby offer to contract for the above-named Work, in accordance with the preceding General Conditions and Specification, at the prices which .... have submitted on the preceding page, and in case .... tender be accepted .... do hereby undertake and agree to execute a Contract in accordance with General Conditions, Clause 10, and .... propose as Sureties as required by that Clause ....

of ....

and ....

of ....

Dated .... day of .... 190 .

Signature ....

Address ....

....

List of Drawings submitted by Tenderer under Section :—

....

....

....

....

FORM OF AGREEMENT.

This Agreement made the day of 190

BETWEEN

(hereinafter referred to as the "Contractor") of the first part the

(hereinafter called the "Purchasers") of the second part and of

and

of

(hereinafter called the "Sureties") of the third part **Whereas** the Purchasers are about to erect and maintain the

hereinafter called

the "Works" mentioned enumerated or referred to in certain General Conditions Specification Drawings Form of Tender and Schedule of Prices and the further Specification entitled "Additional Details" which for the purpose of identification have been signed by

on behalf of the Contractor

and (the Engineer of the Purchasers) on behalf of the Purchasers **And Whereas** the Purchasers have accepted the Tender of the Contractor for the provision and execution of the

said works for the sum of

upon the terms and subject to the conditions hereinafter mentioned **And Whereas** the Sureties have agreed for the consideration hereinafter appearing to enter into the covenants hereinafter contained and on their part to be performed: **Now this Indenture Witnesseth** that in pursuance of the said Agreement and in consideration of the payments to be made to the Contractor by the Purchasers as hereinafter mentioned the Contractor hereby covenants with the Purchasers their successors and assigns that he shall and will duly provide erect and complete uphold and maintain the Works mentioned enumerated or referred to in the Contract and shall do and perform all other works and things therein mentioned or described or which are implied therefrom or therein respectively or may be necessary for the completion of the said Works within and at the times and in the manner and subject to the terms conditions and stipulations in the Contract mentioned and to the satisfaction of the Engineer for the time being

of the Purchasers and also will to the like satisfaction maintain the same in an efficient manner as mentioned in the Contract and shall and will observe and perform all the conditions and provisions set out in such Contract and that all the powers liberties rights and privileges mentioned therein and conferred thereby in respect of such Works shall and may be exercised according to the true intent and meaning thereof **And** in consideration of the due provision erection execution construction and completion of the said Works and the maintenance thereof as aforesaid and of the covenant of the Sureties hereinafter contained the Purchasers do hereby for themselves their Successors and assigns covenant with the Contractor that they the Purchasers their successors and assigns will upon the certificates of the Engineer for the time being of the Purchasers pay to the Contractor the said sum of

or such other sum as may become payable to the Contractor under the provisions of the Contract such payments to be made at such time and in such manner as is provided by the Contract. And the Sureties at the request of the Contractor and in consideration of the Purchasers entering into this agreement do hereby jointly and severally covenant and guarantee with and to the Purchasers that the Covenant on the part of the Contractor in this Contract contained shall be well truly and faithfully performed by the Contractor in every respect according to the true intent and meaning of this Contract and that in the event of default on the part of the Contractor in respect of the performance in any particular of the said Contract the Sureties will pay to the Purchasers all such losses damages costs charges and expenses as the Purchasers may sustain incur or be put unto by or by reason or in consequence of any such default but so nevertheless that the total amount to be demanded or recovered by the Purchasers of or from the Sureties shall not exceed the sum of Ten per cent. of the total contract price.

Provided always and it is hereby covenanted agreed and declared between and by the parties hereto that these presents are entered into and the said Works are to be provided erected executed constructed completed and maintained upon and subject to the terms and conditions contained in the Contract **And** that the parties hereto respectively shall have such rights powers and liabilities and the said Engineer shall have such powers and authorities in respect of the said Plant and the tools and materials for the same and extension in respect of the Contract and all matters connected therewith as are given and expressed by and in the same terms and provisions of the Contract.

In Witness whereof etc.

INDEX TO GENERAL CONDITIONS.

160. INDEXED ISSUED MARCH, 1903. VOL. 32 [PART 2].

JOURNAL

OF THE

INSTITUTION OF ELECTRICAL ENGINEERS,

LATE

THE SOCIETY OF TELEGRAPH-ENGINEERS AND ELECTRICIANS.

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JOURNAL

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VOL. 32. 1903. No. 160.

The Three Hundred and Eighty-Sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 22, 1903 [1]—Mr. JAMES SWINBURNE, President, in the Chair.

The minutes of the Ordinary General Meeting held on January 8, 1903, were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that their names should be suspended in the Library.

The following transfers were announced as having been approved by the Council :—

From the class of Associate Members to that of Members—

Arthur Brunel Chatwood.
Henry Mannington Sayers.

From the class of Associates to that of Members—

Albert Wilson Jones.
Arthur Jas. Stubbs.

From the class of Associates to that of Associate Members—

F. Biliotti.
Hy. Louis Victor Joly.
Walter James Leeming.
John Mark Auguste Margetts.
F. Tandy.
Edward Stanley Shoults.

From the class of Students to that of Associates—

William Gilbert Carter.
Geo. William Selwyn Driver.
Geo. Marinier.
Albert Henry Midgley.

The President announced that, owing to a misunderstanding, Mr. Graham T. Olver's name had been printed in the list of Associates,

[1] The discussion on the Metric System intervened between the reading of Mr. Scott's and Mr. Esson's papers on the 8th of January, and the discussion upon them ; but in order to preserve the continuity of the record in the Journal, the report of the meeting of the 8th of January has been printed after that of the meetings on the 22nd of January and the 5th of February (*vide* p. 328).

instead of in the list of Members, published in 1902, and that his name had now been restored to the Register of Members.

Messrs. J. O. Girdlestone and C. O. Grimshaw were appointed scrutineers of the ballot for the election of new members.

Donations to the *Building Fund* were announced as having been received since the last meeting from Messrs. A. W. Beuttell, I. Braby, J. B. Braithwaite, S. L. Brunton, A. Burton, G. B. Byng, Major P. Cardew, J. B. Edwards, C. F. Farlow, S. Z. de Ferranti, C. G. Friedelberg, R. F. Fuller, H. E. Harrison, D. Henriques, H. Hirst, P. Hunter Brown, E. Hutchinson, W. M. Mordey, D. S. Paxton, C. W. D. Peel, C. Poulsen, E. R. Rudge, P. W. Sankey, K. W. Sutherlands, A. D. Williamson, and L. Wood; and to the *Benevolent Fund* from Messrs. I. Braby, Major P. Cardew, H. C. Donovan, D. Henriques, W. J. S. Pyper, and E. B. Thornhill, to whom the thanks of the meeting were duly accorded.

The PRESIDENT: This evening we shall make a somewhat unusual arrangement. Nominally we are to have a discussion, but as far as we can we shall follow our practice of treating the first part as a paper: that is to say, I shall call upon Mr. Siemens to open the proceedings by reading his paper, and after that I shall call on Sir Frederick Bramwell to open the discussion in the form of a reply to Mr. Siemens; after that there will be a general discussion, and Mr. Siemens will be given an opportunity of summing up at the end.

I have now much pleasure in calling on Mr. Alexander Siemens to read his paper on the Metric system.

NOTES ON THE METRICAL SYSTEM OF WEIGHTS AND MEASURES.

By ALEXANDER SIEMENS, Past President.

On November 14th, 1783, James Watt wrote to his friend, Mr. Kirwan, about the trouble he had experienced in reducing the weights and measures, when comparing the experiments made by Lavoisier and Laplace with results obtained by Mr. Kirwan, and he continues:

> "It is, therefore, a very desirable thing to have these difficulties removed, and to get all philosophers to use pounds divided in the same manner, and I flatter myself that may be accomplished, if you, Dr. Priestley, and a few of the French experimenters will agree to it; for the utility is so evident that every thinking person must immediately be convinced of it. My proposal is briefly this:
>
> Let the philosophical pound consist of 10 ounces or 10,000 grains.
> " ounce " " 10 drachms or 1,000 "
> " drachm " " 100 grains or 100 "
>
> Let all elastic fluids be measured by the ounce measure of water, by which the valuation of different cubic inches will be avoided, and the common decimal tables of specific gravities will immediately give the weights of those elastic fluids."

After discussing the claims of various pounds, he concludes the letter by saying :

> "Dr. Priestley has agreed to this proposal, and has referred it to you to fix upon the pound, if you otherwise approve of it. I shall be happy to have your opinion of it as soon as convenient, and to concert with you the means of making it universal.—I remain, etc.
>
> "I have some hopes that the foot may be fixed by the pendulum, and a measure of water, and a pound derived from that ; but in the interim let us at least assume a proper division, which from the nature of it must be intelligible, as long as decimal arithmetic is used."

On November 23rd, 1783, James Watt wrote to M. de Luc on the same subject :

> ". . . Indeed to compare one experiment with another even where the weights used are the same, gives much trouble from the absurd subdivisions used by all Europe ; and also to compare cubic inches of various substances with weights is a perpetual source of unnecessary calculation ; in order to avoid which I proposed to Dr. Priestley and Mr. Kirwan to agree on a perpetual decimal subdivision of the pound thus :
>
> 100 grains = 1 drachm ; 1,000 grains = 1 ounce ;
> 10,000 grains = 1 pound.
>
> All the elastic fluids to be measured by the ounce or pound measure. The decimal tables of specific gravities will give the weights without calculations. All liquids to be weighed. Mr. Kirwan answers that Mr. Whitehurst is at work on a philosophical measure, from which he means to deduce a pound, divided as above ; but I say, as it may be long before that comes forth, let the expedient of the proper division take place in the meantime. Dr. Priestley will immediately adopt it, and I will be obliged to you to write to M. de Laplace on the subject. In order to introduce uniformity as much as we can, we mean to subdivide the Paris pound in 10,000 parts . . ."

These two letters have a special bearing on the subject which is to be discussed to-night, as Watt had laid down in them the fundamental conditions on which the metrical system is based.

It is even probable that Watt is directly responsible for the movement among French scientific men, for his biographer, Muirhead, tells us that in 1786 Watt and his partner, Boulton, went to Paris, and there "they had the satisfaction of making the acquaintance of most of the eminent men of science, of whom the great capital of France had then to boast, as Lavoisier, La Place, Monge, Berthollet, De Prony, Hassenfratz, Fourcroy, Delessert and others." No doubt Watt's idea of a "philosopher's pound" was discussed among them; at least it bore fruit, for in the year 1790 Prince Talleyrand proposed to the Constituent Assembly of France that the many systems in use in that country be *changed into one system*, and that be a decimal one founded

on the pendulum. This was adopted by the Assembly on the 17th of March, 1791, and sanctioned by Louis XVI.

It will be noted that this plan is foreshadowed in the postscript of Watt's letter to Kirwan, and a further indication of his influence may be traced in the provision that the French Academy and the Royal Society of Great Britain appoint jointly an International Commission for discussing the subject of universal weights and measures.

England declined, however, to co-operate, but Spain, Italy, the Netherlands, Denmark, and Switzerland were finally represented on this Commission, which consisted of the ablest mathematicians then living.

The system, suggested by Watt and adopted by this International Commission, derives the units of weight and of capacity from the linear standard, and the chief object of the Commission was to settle how the linear standard was to be fixed.

Three linear standards were discussed:

1. The length of a pendulum beating seconds.
2. The length of a quadrant of the equator.
3. The length of a quadrant of a meridian.

Eventually the last was selected, and it was decided that the ten-millionth part of this quadrant should be the linear unit, "the meter."

A law, passed on August 1st, 1793, established the system provisionally, and the nomenclature was sanctioned nearly two years later on April 7th, 1795.

For seven years the survey of the meridian between Barcelona and Dunkirk went on, until in 1799 representatives from ten countries assembled in France to examine the results of the survey, and to settle "a definite meter."

When this had been accomplished, Laplace explained the whole system to the legislative councils of France, and it was definitely adopted by a law promulgated on June 22nd, 1799.

Unfortunately, the succession of wars, undertaken at first by the Republic, and afterwards by Napoleon I., against all the other nations of Europe, was then in progress; moreover, Napoleon, personally, did not approve of the change, so it came about that an intermediate system of divisions and of names was tolerated by a law passed on May 28th, 1812.

The pure metrical system was not enforced in France until January 1st, 1840.

Other European countries were at first very reluctant to adopt the metrical weights and measures, but when the inter-communication between distant parts of the same country and between different countries developed during the nineteenth century, the want of uniformity in weights and measures grew more and more inconvenient.

In Germany, for instance, a Commission was appointed to settle a national unit of weights and measures, and it began its investigations in 1861. It was very soon decided, however, not to elaborate a national
by which the recommend the metrical system. The actual intro-
and the common by the wars of 1864 and 1866, and it was only in
ately give the weights was passed making the use of metrical weights

and measures optional from January 1st, 1870, and compulsory from January 1st, 1872.

While the German Commission was deliberating, a Select Committee was appointed by the House of Commons, and it reported in 1862 that in its opinion "it would involve almost as much difficulty to create a special decimal system of our own, as simply to adopt the metric decimal system in common with other nations. And if we did so create a national system we would, in all likelihood, have to change it again in a few years, as the commerce and intercourse between nations increased, into an international one."

In 1864 an Act was passed allowing the use of the metric system of weights and measures, and in 1868 a Bill was brought in to make this system compulsory, but the Bill was dropped after passing the second reading.

The Weights and Measures Act (1878) authorised the Board of Trade, by Clause 38, "to verify metric weights and measures which are intended to be used for the purposes of science or of manufacture or for any lawful purpose, not being for the purpose of trade within the meaning of this Act."

The provisions of this clause became more and more irksome, and another Select Committee was appointed in 1895, which, after examining numerous witnesses for and against the introduction of the metric system, recommended :

"(*a*) That the metric system of weights and measures be at once legalised for all purposes.

"(*b*) That after a lapse of two years the metric system be rendered compulsory by Act of Parliament.

"(*c*) That the metric system of weights and measures be taught in all public elementary schools as a necessary and integral part of arithmetic, and that decimals be introduced at an earlier period of the school curriculum than is the case at present."

In consequence of this recommendation Parliament passed the Weights and Measures Act (1895), but this gives effect only to the first part of it, and we are still waiting for the Act to make the adoption of the metrical weights and measures compulsory.

The only practical steps towards the introduction of the metric system into the United Kingdom have been taken by the British Association, of which a Committee worked out the c.g.s. system of electrical units.

It is not necessary to say in this assembly that these units were subsequently adopted by the International Electrical Congress of Paris (1881), and that their general introduction into all countries has been one of the principal causes of the rapid development of the application of electricity for industrial purposes.

Another Committee of the British Association, consisting of Sir Joseph Whitworth, Sir Wm. Thomson (now Lord Kelvin), Sir F. J. Bramwell, Mr. A. Stroh, Mr. Beck, Mr. (now Sir) W. H. Preece, Mr. (now Colonel) R. E. Crompton, Mr. E. Rigg (secretary), Mr. A. Le Neve Foster, Mr. Latimer Clark, Mr. (now Sir) H. T. Wood and Mr. Buckney

was appointed for the purpose of determining a gauge for the manufacture of the various small screws, used in telegraphic and electric apparatus, in clockwork and for analogous purposes.

After deliberating for two years this Committee recommended in 1884 the adoption of the Swiss series of small screws, commencing with the pitch of one millimeter, and decreasing the pitch of each succeeding size by 10 per cent.

In the United States by an Act of Congress, approved in July, 1866, the use of the weights and measures of the metric system is made permissible and the "international prototype meter and kilogramme" (deposited in Paris) are regarded as the fundamental standards of length and mass; and the yard and pound are to be derived from the metric standards.

It is proposed to bring in a Bill during the present session of Congress directing all Government departments to use the metrical system for all their transactions from January 1st, 1904, and making the system compulsory throughout the United States from January 1, 1907.

About other civilised states, apart from Great Britain and the United States, it is only necessary to say that they have adopted the metrical system on account of the simple relations between the units of length, of weight, and of measure, and on account of the decimal subdivision of the units which agrees with the arithmetical notation used universally.

The metrical system is, of course, not the only one by which these two advantages can be obtained; for instance, Sir John Herschel suggested a rival system by making the polar radius of the earth the unit of length.

This radius he estimated to be 500,500,000 inches long, and he suggested that the English inch should be increased by its $\frac{1}{1000}$ part, so that it should be exactly the 500 millionth part of the polar radius.

He then undertook to show that by increasing the grain (by legislative measure) by its $\frac{1}{18}$ part a cubic foot of water would weigh a thousand ounces.

> "And thus the change, which would place our system of linear measure on a perfectly faultless basis, would at the same time rescue our weights and measures of capacity from their present utter confusion, and secure that other advantage, second only to the former, of connecting them decimally with that system on a regular, intelligible and easily-remembered principle; and that by an alteration practically imperceptible in both cases and interfering with no one of our usages and denominations."

In this proposal Herschel committed the same error as the compilers of the metrical system by adopting a terrestrial dimension as a "perfectly faultless" basis for his linear standard; in both cases later measurements with improved instruments have proved that the original results were not accurate.

With regard to the decimal subdivision of the units, Sir George Airy, the late Astronomer Royal, said :

> "It appears to me that the practice of mankind, as regards their selection of scales of multiples and subdivisions, in every subject which I have examined, may be described thus : For each particular subject to which measure, etc., is applied some one measure, etc., is adopted as the standard. Then the multiples of this measure, etc., are taken on the decimal scale, and the subdivisions are taken on the binary scale. These subdivisions are taken without any regard to their coincidence or non-coincidence with inferior measures, etc. The coasting sailor uses the league, $\frac{1}{2}$ league, $\frac{1}{4}$ league, without regard to miles or yards. The traveller uses the mile, $\frac{1}{2}$ mile, $\frac{1}{4}$ mile, furlong, and never combines them with the yard or the foot. The sailor, in sounding, uses the fathom, $\frac{1}{2}$ fathom, $\frac{1}{4}$ fathom, and thinks of no other measure. The vendor of drapery uses the yard, $\frac{1}{2}$ yard, $\frac{1}{4}$ yard, etc., down to the nail, without regard to inches. The joiner uses multiples of inches to a large number and subdivides the inch to $\frac{1}{4}, \frac{1}{2}, \frac{1}{8}, \frac{1}{16}$. . . . It is very little important whether the relation between the standards adopted for the different measures (for instance, the mile and the yard) be or be not simple, provided that it be ascertained."

Then Sir George Airy also said in his evidence before the Select Committee of 1862 :

> "If I had a new nation to create, with a new style of weights and measures, I would give them the binary scale throughout ; that I consider nearest perfection, with means to enable us to use decimal multiples and sub-multiples."

Curiously enough, these statements of Sir George Airy were laid before the Select Committeee of 1895 by Mr. Stevenson as adverse to the metrical system.

A little reflection will, however, convince everybody that the metrical system fulfils all the requirements laid down by Sir George Airy, with the additional advantage that the standards for the different measures stand in a simple decimal relation to each other.

For instance, a traveller might use a km., $\frac{1}{2}$ km. and $\frac{1}{4}$ km. ; a joiner will use millimeters or $\frac{1}{2}$ and $\frac{1}{4}$ mm. ; a merchant kg. for ordinary transactions, and tons for larger transactions, subdividing them on the binary scale, whenever he finds that convenient.

In most transactions, according to metrical weights and measures, no fractions are necessary, either vulgar or decimal, as the smallest units, the milligramme and the millimeter, need not be subdivided for ordinary purposes, and their multiples can be readily expressed in higher units, if that should be desired.

For this reason it may be anticipated that the introduction of the metrical system would cause no inconvenience ; especially not in the retail trade.

The experience of other countries, which have made the change to the metrical system, proves that the initial difficulties of the transition from the old weights and measures can easily be met by providing conversion tables, which should be displayed in conspicuous positions in all shops.

It so happens that the principal changes which concern the retail trade, viz., from the yard to the meter, and from the lb. to the ½ kg. (metrical pound), are very simple.

The meter is about $\frac{1}{11}$ longer than the yard, the cost per meter is, therefore, 1d. per shilling greater and the metrical pound is a little more than 10 per cent. heavier than the pound avoirdupois, so that the cost of the ½ kilo. exceeds the cost of a pound avoirdupois by 1¼d. in the shilling.

If the customer, therefore, does not wish to rely on the conversion table, he can calculate the price, as if he were dealing with yards or pounds, and then add in the first case 1d. for each shilling of the result and in the case of the pounds 1¼d. for each shilling.

Although such a conversion is not absolutely accurate the difference is much smaller than the fluctuations of wholesale prices.

Again, the coil of 110 yards is only 23 inches (or 0·58 per cent.) longer than 100 meters, so that the substitution of the one for the other need not cause any inconvenience.

In spite of all these facilities it is only natural that the use of new weights and measures, as long as they are unfamiliar, will contrast unfavourably with dealings in the old weights and measures, but the public will soon find out what a blessing the abolition of the various and perplexing tables of weights and measures will prove to be, and how superior the metrical system is.

This opinion was tersely expressed by Mr. Balfour, when he said to a deputation which urged him to carry out the recommendation of the Select Committee of 1895:

> "Upon the merits of the case I think there can be no doubt whatever, that the judgment of the whole civilised world, not excluding countries which still adhere to the antiquated systems under which we suffer, has long decided that the metric system is the only rational system."

sident.

The PRESIDENT: I now call on Sir Frederick Bramwell to open the case for the other side.

Fredk. mwell.

Sir FREDERICK BRAMWELL, Bart., F.R.S.: Mr. Siemens starts off with the history of the metrical system, about which I have nothing to complain. He says it is due to the most eminent mathematicians of the day. You will find that Napoleon had something to say upon that point. The law was passed in June, 1799; and Mr. Siemens attributes to the continued wars and to the ill-will of Napoleon the fact that the law did not come into operation until 1840—forty years later. I can remember very well its coming into operation, and I remember it by reason of a bad joke which often makes a very good memory point for me. There was a French paper then published in London called the *Courier de l'Europe*, to which I subscribed, and they pretended that a gentleman whose name was Mr. Vingtsous had written that name over

Sir Fredk. Bramwell.

his shop, and was haled before the magistrate for having infringed the law. He said: "What am I to do? I am going to be married to-morrow. What am I to call myself; what ought I to have written up?" "You ought to have written up Franc, and not Vingtsous." "Am I to call myself Mr. Franc when I marry!" That is why I remember when the law came into force. The use of the system was permitted in England in 1864, and the Act of 1878 allowed the Board of Trade to verify the weights and measures. There was a Committee appointed in 1895 to consider the metric system; this Committee has been alluded to by Mr. Siemens. I have a complaint to make against the Committee. A gentleman came forward and gave a calculation of a sum carried out metrically, and also in the way he was pleased to call the ordinary way. It could only have been done from gross ignorance or from malice prepense. I put in a counter calculation of the true ordinary way; and, whereas he had succeeded in showing that according to his ordinary way the number of figures employed exceeded those used in the decimal mode by, I do not know what multiple—double, I think—I showed that according to the real ordinary way the number of figures employed was half the decimal mode number of figures. That calculation the Committee refused to publish; it is not there in the evidence, and I had recourse to the *Times* to get it before the public. Now this is a little bit of over-zeal which I think it might have been well to have left out. The Committee came to the three conclusions stated by Mr. Siemens: (*a*) That the metric system of weights and measures be at once legalised for all purposes. (*b*) That after a lapse of two years the metric system be rendered compulsory by Act of Parliament. (*c*) That the metric system of weights and measures be taught in all public elementary schools as a necessary and integral part of arithmetic, and that decimals be introduced at an earlier period of the school curriculum than is the case at present. We have now, in the Act of 1897, arrived at the outcome, so far, of that Committee's conclusions. What says that Act? Stated shortly, it says: "Notwithstanding anything in the Weights and Measures Act, 1878, the use in trade of a weight or measure of the metric system shall be lawful, and no person by reason of using or having in his possession a weight or measure of the metric system shall by reason thereof be liable to a fine. The Board of Trade standards, which may be made under Section 8 of the Weights and Measures Act, 1878, shall include metric standards derived from the original platinum linear standard metre, and original platinum standard kilogramme deposited at the Board of Trade and numbered 16 and 18 respectively. It shall be lawful for the Queen by order in Council to make a table of metric equivalents." In the following year the Council did make a table of metrical equivalents, and here they are, but I do not think I can usefully take up your time by reading them to you. That is what has come of the recommendation of the Committee of 1895 up to the present moment. Now Mr. Siemens very frankly says that his desire is that letter (*b*) of the Committee's recommendation—that after a lapse of two years the metric system be rendered compulsory by Act of Parliament—should be adopted. That really is the *question before* the meeting to-night. Perhaps Mr.

Sir Fredk. Bramwell. Siemens has seen a copy of the proposed Bill. I have not; I can only judge from that of 1868. Now the Bill was not to be a mere matter of words; it was to be very stringent and very troublesome indeed. Section 10, stated shortly, provided that "Every person who shall sell otherwise than by the metric system shall be liable to a penalty not exceeding 40s. for each sale." Section 11: "Every person who shall print, who shall make any return as Clerk of the Market, price list, price current except metric shall pay any sum not exceeding 10s. for each copy of such paper." Therefore the unhappy gentleman who had made a blunder of that kind might very soon, if he had printed a thousand papers, find himself fined £500! That is the kind of thing we may expect, I suppose, and I do not say it is unreasonable from the point of view of the "Metrics"; from the point of view of a gentleman who believes that you could not go to heaven if you did not profess certain things, it is not unreasonable to torture your body, because your soul is worth more than your body and the benefit to the soul outweighs the bodily torture. But this kind of thing, you know, does away with the question of the survival of the fittest. I thought in these days we had come to regard the survival of the fittest as being the absolute test of the value of any system. The advocates of the metric system can now, after this Act of 1897, do what they like—have their weights, keep their accounts, do everything that they please. "No," they said, "that will not do for us. You shall use the metric system, whether you like it or not." That is not the survival of the fittest, and it is inconsistent with anything like liberty. The only thing of which it reminds one is the now somewhat old tale of the man who said "Sir, this is the freest country on earth; every man does as he likes, and if he does not we make him." Mr. Siemens goes on to state that the system is now pretty nearly adopted all over Europe except in Russia. I am sorry to say that is so, but so it is; I cannot contradict it. [Mr. SIEMENS: These clauses are taken from the other Acts; they are taken from the Weights and Measures Acts which are in force, enforcing the English weights. It is the same clause.[1]] One can see why Germany adopted the metric system. Germany was made up of a number of States—so was Italy, by the bye—with divers laws and regulations. They were compelled to come to some one system, and no one of those small States could have devised a system which would have been taken up by the whole Empire; therefore they were glad to adopt that system which was to their hand. I am surprised to find that Mr. Siemens has not put forward the usual plea, which, to my mind, is the only one that has even the shadow of a value in it, namely, that our merchants and manufacturers are handicapped in their dealings with foreign nations because the foreign nations do not understand our measurement—they know about our coinage—in which we deal with them. Whose fault is that? Is it not the fault of the merchant? He may do what he likes with the metric.

[1] (*Note added January 29th.*)—The reply to the above remark of Mr. Siemens is that so long as such clauses applied to existing weights and measures, there was but little chance of them being infringed, and therefore little chance of the penalties being imposed; but, when applied to the introduction of a new system, both infringement and penalty would follow, as a matter of course, for years to come.

No, that will not do for him. He says "No; all of you shall be compelled to deal in metric; I am not going to do it by myself, but you shall all do it." That kind of answer is so obviously wrong, and the suggestion that the whole difficulty can be met by the merchants themselves so very clear, that I am not surprised Mr. Siemens did not put this stock objection forward; he knew the comment that the merchants had the remedy in their own hands was unanswerable. Sir Fredk. Bramwell.

Mr. Siemens says that the Bill of 1868 was dropped, and refers to Sir George Airy and Sir John Herschel. I am inclined to think that Mr. Siemens has misquoted both those gentlemen—quite unintentionally, I am sure. Sir George Airy is being examined before the Committee of 1862 and very much pressed by the Chairman. Mr. Siemens has quoted one answer: "If I had a new nation to create, with a new style of weights and measures, I would give them the binary scale throughout." That does not look much like decimals; he then goes on—I cannot quite understand what he means—"That I consider nearest perfection, with means to enable us to use decimal multiples and sub-multiples." But he would start with the binary scale. That was in answer to Question 1968. Mr. Siemens did not quote the previous Question, 1967, when the Chairman endeavoured to force Sir George Airy's assent to the decimal system. This is the question and answer: "*Q.* But if the change"—that is, to the decimal system—"could be made *per saltum* and you could be transferred from your present state into the decimal system, do not you think the change would be advantageous? *A. No.*" That does not look very much like Sir George Airy being an advocate of the decimal system—the question of the metric system did not then appear to arise.

Now as regards Sir John Herschel. Writing on the 6th of April, 1868, to Mr. Beresford Hope on the Bill of 1868 for the compulsory use of the metric, he said, "Pray pardon me for calling your attention to this Bill of Ewart's . . . . in the hopes that you will oppose it at all events by vote, and perhaps by words." That does not look like Sir John Herschel being an advocate of the metric system. Beresford Hope did oppose it. I wish that time permitted me to read the whole speech to you, but it does not. He complains of the proposed compulsory use of decimals, and then still more earnestly of the compulsory use of the metric system, with its Greek and Latin terms. Although I cannot read the whole speech, may I read the following extracts:—

EXTRACT FROM "HANSARD'S PARLIAMENTARY DEBATES," VOLUME 192.

"But I may be told—Halve away, but then express your halvings in decimals. This is very easy for the merchant prince to do when he is totting up his large transactions in 'centals,' or for the Chancellor of the Exchequer when dealing with a nation's finances; but how will it suit the little transactions of daily life? I come back to my loaf. How are ordinary people to represent halves and quarters by decimal points? The symbol of a half is the figure 'five,' with a dot to its left hand; the symbol of half that quantity —that is of a quarter—is the sum twenty-five, also with a dot to its left hand. Arithmeticians understand how this can come about, and the symbols have grown natural in their eyes; but in what—even the most infinitesimal —degree *do they tell their own* story to the unlearned? What palpable

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relations towards each other can be disentangled out of these most frequently recurring symbols? What is there in the nature of things to show that the dotted five means a half, and the dotted twenty-five a half of that half, and a quarter of the 'one,' with no dot on either side, which stands for unity? Decimal notation is then, after all, as I have been arguing, a process, and not a system. It is a process good for the schools, and good for the bustling counting-house and the large sum, but the poor man would be completely thrown out if he had to employ—under penal legislation, too—decimal points for the purpose of measuring his little purchases by halves and quarters."—(Page 188.)

"One-tenth part of this gram is to be a decigram, and ten times a gram is to be a dekagram, for the reformers decreed that aliquot parts were to be named after the Latin, and multiples after the Greek numerals. How in the name of common sense can we make poor people understand that because there are the letters 'ci' in the one word it means the tenth of a gram, and that because there are the letters 'ka' in the other it means ten grams, or 100 decigrams? My hon. friends the Member for Dumfries and the Member for Liverpool come to this House representing great commercial transactions; but I stand up for the poor man. Only imagine an honest housewife going into a shop and asking for a decigram of pepper, and a dekagram of tea; imagine, too, the milkmaid selling her fluid by the litre. The Member for Liverpool is a kind-hearted man; is he then prepared, with all the stringent force of a penal statute, to enact that when one of his youthful constituents may desire to effect a commercial transaction in a manufacture for which one portion of that great borough is famous, he should be bound to go to the shop and tender his 'dime' for three decigrams of Everton toffee? Fancy the farmer, who has been accustomed ever since he entered on his farm to cultivate the 'ten-' or the 'twelve-acre field,' having to consult the steward about liming the seventeen-are field, or be a criminal and a contemner of the laws of his country. Fancy the bumpkin who was prepared to boast that he was within a decimeter of catching the fox as he crept through a gap about a dekameter from the white gate. If the theorists and the men of wealth—men of brains, it may be, but as certainly men of self-assurance—have worked out this system for themselves, there are poor men, who form the majority of mankind, for whom it will never answer, and there are men of brains at least equal who are decidedly opposed to its adoption."—(Page 189.)

I think this speech contains about the best common sense, and is one of the most convincing speeches I have ever read.

Mr. Siemens wants the compulsory metric system pure and simple. He has not dealt to any extent with the decimal question; but the decimal question being an inevitable part of the metric system, I must deal with decimals. I believe a good many of my friends imagine I am an opponent of decimals. I am nothing of the sort; I cannot do without them; I should be greatly troubled to extract a square root or cube root without them; and without them I could not use logarithms. I want this to be borne in mind: that of which I am an opponent is not decimals, but the compulsory use of them on all occasions. It is that to which I object. Modes of calculation are only tools to attain an end. Imagine a carpenter with a bag of tools, including an adze, meeting an enthusiast about what can be done with an adze. The enthusiast says to the carpenter: "You shall use that adze on all occasions." "Oh," the carpenter says, "nonsense; I have other tools which at other times are more convenient." "I do not care; it shall be illegal if you use any other tool than an adze." I suppose that anybody who brought that forward would be promptly sent to Bedlam. It is the same thing with the obligation to use decimals on all occasions. Remember, a decimal is nothing but a vulgar fraction with a denominator of always

one kind; you do not write the denominator, but you indicate by the position of the decimal point, if you know how, what the real denominator would have been if you had written it. You have one more figure in the denominator than you have up above. When you get a point and the figure (·1), there is in the denominator one more figure than the 1, viz. the 0, and you would write as a vulgar fraction $\frac{1}{10}$; the denominator has two figures, that is one more than the numerator, which has only one figure. When you write decimally a $\frac{1}{100}$ you put ·01; you have one more figure in the denominator than you have in the numerator above, and so on. Very much practice is needed to insure the accuracy of the point before the significant figure; I think most of you who have had anything to do with the decimal calculations will admit that. Sir Fredk. Bramwell.

May I worry you with one or two instances. In 1885 a paper was read in this room before the Institution of Civil Engineers. A large illustrated diagram was put up, which was illustrative of decimals. I found that the decimal point was in the wrong place. That was a specimen diagram!

Then I had a correspondence in the *Times* some two or three years ago with a Frenchman stopping in London upon questions of decimals, and he chaffed me in his letter at my clumsy mode of taking off 2½ per cent. What do you think he said he would do? He said, "All that I should have to do would be to multiply by 97·5." I said that all I had to do was to take off $\frac{1}{40}$th—2½ per cent.

I will give you another case that came under my own knowledge; I did not bring the papers here, but I have re-looked at them. In 1897 there was an arbitration between a Water Company and a Local Board; I was umpire in the ascertaining of the sum to be paid. There had been a previous hearing to find out whether the Company was in default on the ground of not being able to give sufficient water. The population of the district was almost exclusively composed of working miners, and among the demands made was one of 4 gallons per head for manufacturing purposes in a population of working miners. The Company were declared in default, and the Local Board stepped in. When they came before me to determine the sum to be paid, the counsel who opened the case for the Company said, blushing, "I am very sorry. There was a great mistake which nobody on either side found out: that 4 gallons was not 4 gallons at all; it was ·4"; that was stated also by the first witness who appeared for the Company. Therefore, here there is an instance of a body of eminent barristers and of eminent engineers working together, and all blundering over the decimal point.

Then there was a communication in the newspaper when the now King was shot at in Brussels; it came from the Continent, and, I believe, from a metric-using country. They reported that the bullet was ·1 metre long. I tried to put that length of metal into a revolver, and found that not only would such a bullet have filled the whole length of the barrel, but that it would have projected to about an equal length. I cannot help feeling that many of us are in the position of the man whom his friend found figuring away to the ninth or tenth *place of decimals*; and when the friend said to him, "Why

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are you taking all this trouble; no such accuracy as this is needed for this case?" he replied, "The fact is I am by no means sure of my first figure on the left hand, but I am determined to cure any mistakes by going on." As I say, I like decimals when they are in the right place, but not otherwise.

On many occasions I find it better, and mankind has found it better, to use $\frac{1}{2}$, $\frac{1}{3}$, and so on; that is to say, that whereas the decimal is a vulgar fraction with a denominator always of the same kind, these vulgar fractions have the numerators always of the same kind, namely, 1, and especially mankind will use these vulgar fractions when the fractions are arrived at by repeated divisions by 2, and will not use ·5, ·25. I remember in a farce there was introduced a clerk who was a joint clerk to four briefless young barristers, who was called by each of them "·25," showing that the decimal system had reached the law. Humanity jibs even at ·5 and ·25; but when you get to $\frac{1}{8}$ (·125) it is a little complex: $\frac{1}{16}$ (·0625) is still worse; $\frac{1}{32}$ (·03125) is "worser"; and $\frac{1}{64}$—all these figures are legitimate divisions and are used in respect of shares in ships and so on—(·015625) is still worse. They are terribly unwieldy, but they are accurate. But when we come to the other fractions—$\frac{1}{3}$, $\frac{1}{6}$, $\frac{1}{7}$, and $\frac{1}{9}$—they are both unwieldy and inaccurate, and yet I am to be compelled to use them when I do not want to do so. The worst case, of course, is the $\frac{1}{7}$ (0·142857). For men like the late G. P. Bidder and Zerah Colbourne, and for some ladies now living, such a mass of figures as these present no difficulty, but to me they are insuperable. I can now only do the most rudimentary mental calculations; when I was younger I could do them pretty well, and I found them of the greatest possible use. Are there any gentlemen in this room who can mentally square 3·142857 ($3\frac{1}{7}$) and give the result in decimals? I should be very glad and very much surprised if any one of you will get up and do it. Suppose I wanted to square $3\frac{1}{7}$, and am allowed to use the vulgar fraction; it is done in a moment: 3 times 3 are 9, 3 times $\frac{1}{7}$ twice over are $\frac{6}{7}$, and $\frac{1}{7}$ of a $\frac{1}{7}$ is $\frac{1}{49} = 9\frac{48}{49}$. I cannot square mentally 4·875, but I can square mentally $4\frac{7}{8}$ ($23\frac{49}{64}$).

In this particular I appeal to all of you who have travelled, as every one has in these days, whether it is not the fact that the clerk at the ticket-office of a foreign railway is compelled on the simplest question of multiple-tickets or of multiple-conditions of ticket, to take a piece of chalk or a pencil and make a calculation before the amount to be paid can be stated. Compare that with an English railway clerk. You say, "Two firsts from A. to B., two thirds, and a child;" and he will mentally calculate it immediately, and accurately. Together with my wife I came from Marseilles to Paris some years ago. I thought that at Marseilles they had charged a very high fare, although I simply took two first-class tickets. I was right; their arithmetic was wrong owing to their upbringing, but their honesty was everything to be desired, for when we reached Paris I was asked, "Did you pay so-and-so at Marseilles?" "Yes," I replied, and I had the extra money returned to me. Their honesty could not be improved, even by vulgar fractions, but with the *vulgar fractions* they would not have made the mistake. Again, take *an English* butcher's wife or daughter: $9\frac{1}{2}$ lbs. of mutton at $10\frac{1}{2}$d.—it is

done in a moment. I shall be glad if any one will tell me what assistance decimals are in any one of the four rules of arithmetic, except in the addition of such matters as £ s. d., or feet and inches. They are no good in division, they are no good in subtraction, and they are the very . . . . in multiplication. No doubt in £ s. d. and feet and inches they do save the mental division by 12 and by 20, but I regret that saving, because it gets rid of a perpetual opportunity of doing a little mental arithmetic, and I deprecate anything that does away with the use of mental arithmetic. Sir Fredk. Bramwell.

I have dealt at length with decimals because they are involved in the metric system. Not only does the metric system trouble one with decimals on all occasions, but you have the new fantastic terms in Greek and Latin to which Beresford Hope alluded. As I have said before, a merchant can adopt the metric system if he pleases, but why is it to be imposed on tradesmen and workmen? I will give you some notion of the magnitude of our ordinary weights and measures. Mr. Chaney, the Warden of the Standards—this, I believe, is the title of the gentleman who, at the Board of Trade, looks after the weights and measures—said before the Committee of 1895 that in the year 1893-4 they had stamped 3½ million individual weights or measures. That gives you some idea of the mere year's output in weights and measures, and what the extent of the change would be and how great the annoyance. It would be an intolerable annoyance, and to my mind even sumptuary laws would be preferable. I would very much rather the Government legislated upon the shape of my hat or the make of it, or upon the material of my coat and the "cut" of it, than that they should legislate upon the way in which I should make my calculations.

Mr. Siemens brought forward some eminent names in his favour, but not their arguments. The scientific man fed on French and German books, the merchant, often a foreigner or of foreign descent, too lazy to use the metric system themselves, want to inflict it upon the whole of us. Mr. Siemens quoted Sir George Airy as being in favour of decimals. I think I settled that matter. Airy said "*No.*" "Would not it be a benefit?" "*No!*" As regards Herschel, I have already said he wrote to Beresford Hope, "For heaven's sake go and speak in the House and stop this Bill for the compulsory use of the metric system."

Now may I refer to Mr. Coleman Sellers. I need hardly tell you that he is the Whitworth of the United States? He tried the metric system for twenty years in one department of his works, and condemned it. Rankine, again, is not a bad name to quote. His feeling in opposition to the metric system was so great that he broke into poetry, the celebrated "Song of the three-foot rule," the concluding lines of which are—

"Oh, bless their eyes, if ever they tries
To put down the three-foot rule."

I cannot help thinking that the second word of the first line must be a typographical error! I beg your indulgence for a short time while I deal with Napoleon. I have felt that his views on the matter, written by General *Comte de Montholon*, were so pertinent that I have taken

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the trouble to extract them from the work, and to have them printed in the French. I have prepared a somewhat loose translation in English, but loose as it may be I think it expresses the real meaning of the French. Napoleon says :—

1. The need of uniformity in weights and measures has been felt throughout all ages ; several times the États Generaux have alluded to it. It was expected the Revolution would achieve this unification.

2. The law needed for this matter was so simple that it could have been written out in twenty-four hours, and could have been adopted and put into practice throughout the whole of France in less than a year. All that was required was to make the Units of Weights and Measures of Paris the only legal units throughout France.

3. The Government, the artisans, had for generations past used these weights and measures. By sending standards to every Commune, and by ordering the Administration and the Tribunals not to recognise any others, this reform would have been carried out without trouble, inconvenience, or coercive measures.

4. The geometers, the algebraists, were consulted in a question which was, in fact, purely one of an administrative character. They thought that the unity of weights and measures should be deduced from some natural order, so that it might be adopted by all the nations.

5. They were of opinion that it would not suffice merely to do good to forty millions of men, they wished the whole universe to participate.

6. They found the mètre to be an aliquot part of the meridian. They proved it (to their own satisfaction), and proclaimed it in an assembly of French, Italian, Spanish and Dutch geometers.

7. A new unit of weights and measures was immediately decreed which neither "fitted in" with the regulations of the Public Services, nor with the "rules and tables" of the manufacturers, nor with the dimensions of any existing machine.

8. Moreover, as a fact, the advantages of this system could not extend to the whole universe. It was impossible. The national spirit of the English and of the Germans was opposed to it.

9. If Gregory VII., in reforming the Calendar, was able to render that reformation universal throughout Europe, it was because this reform was connected with religious ideas ; that it had not been made by a nation, but by the power of the Church.

10. Thus the comfort of the present generation was sacrificed to abstraction and to vain hopes, because, for an old nation to adopt a new unit of weights and measures, it is needful to remake all rules of public administration, all the calculations used in the arts. Such a work alarms the reason.

11. The new unit of weights and measures, such as it was, had an ascending and a descending scale, which did not tie in by simple numbers with the scale of the units of weights and measures which for centuries has sufficed for the Government, the scientific men, and the manufacturers.

12. The translation cannot be made from one to the other system, because that which is expressed by the most simple numerals in the old system demands, in the new, composite figures.

13. It became necessary, therefore, either to increase or diminish, by some fractions, in order that the measurement, or the weight, when expressed in the new nomenclature, should be in simple figures.

14. Thus, for example, a soldier's ration is expressed in the ancient nomenclature as 24 oz. This is a very simple expression ; translated into the new, it gives 734 grammes, 259 milles.

15. Thus it is evident that to arrive at the whole numbers (734 or 735 grammes), there must be augmentation or diminution.

16. All the pieces and lines relating to architecture ; all the tools and the parts used in clockwork, in jewellery, in publishing, in all the arts, in all the instruments, in all the machines, had been studied and calculated in the ancient nomenclature, and are expressed by simple numbers, while the translation needed numbers composed of five or six figures ; thus all must be done

over again. The "savants" conceived another idea, altogether nullifying the benefits of unity of weights and measures, for they adopted in their scheme the decimal numeration; they took the mêtre as their unit, and they suppressed all other starting points. Sir Fredk. Bramwell.

17. Nothing can be more contrary to the organisation of the mind, the memory, and the imagination.

18. A fathom, a foot, an inch, a line, a point ($\frac{1}{12}$th of a line), are fixed portions of length, which the imagination readily conceives independently of their relation to one another. If, therefore, one asks for one-third of an inch, the mind operates immediately; it is a portion of length called an inch which is to be divided into three equal parts.

19. By the new system, on the contrary, it is not the operation of dividing an inch into three which has to be performed by the mind; but it is the division of a mêtre into 111 parts. The experience of ages has shown the difficulty of dividing a distance or a weight beyond 12, for at each of these divisions there has been created a new "term" or starting point.

20. If one wanted the $\frac{1}{12}$th part of an inch, the operation was already made; it was the "term" "ligne."

21. Decimal numeration could (under the old system) be applied to all the "terms," and if an hundredth of a "point" or of a "ligne" was required, one wrote $\frac{1}{100}$th; but if one wishes to express an hundredth of a "ligne" by the new system one has to consider its relation to the mêtre, which causes endless calculation.

22. The divisor 12 has always been preferred to the divisor 10, because 10 has only 2 factors—2 and 5; while 12 has 4, viz., 2, 3, 4 and 6. It is true that the decimal numeration, generalised and exclusively adapted to the mêtre, as unity, affords facilities to astronomers and to mathematicians, but these advantages are far from compensating for the inconvenience of rendering the thinking-out more difficult.

23. The first object of every method should be to aid conception and imagination, to assist the memory, and to give greater power to the mind.

24. The various "terms" are as old as man, because they are in the very nature of his organisation, just as it is in the nature of decimal numeration to adapt itself to each unity, and to each "term," and not to one unit exclusively.

Further, these scientific men used Greek roots, thereby augmenting the difficulty. Such denominations, though they may be serviceable to the scientists, are not good for the public.

The "Weights and Measures" were among the great affairs of the Directoire.

Instead of leaving it to the influence of time, and of contenting themselves with encouraging the new system, by means of example and of custom, they make coercive laws, which were executed with vigour.

The merchants and the people found themselves harassed for that which was in fact a matter of indifference. This contributed still further to make unpopular an administration which placed itself aloof from the wants and the powers of the people; which violently broke their practice, their habits and their customs; just as might have been done by a Greek or Tartar conqueror, who, with the rod of power uplifted, enforced obedience to his will, who commanded according to his prejudices and his interests, regardless of those of the vanquished.

The new system of weights and measures will be the source of embarrassment and difficulty for many generations, and it is probable that the first scientific commission to whom it is given to verify the measurements of the meridian will find some corrections to make. It is a tormenting of the people for mere trifles.

I have the honour entirely to agree with Napoleon upon those points.

Now, what are the facts? The intended enumeration was meant to be the millimetre expressed by 0·001. But this is not used. People write m.m. Again, the centimetre: nowadays they do not write 0·01, but when dealing with capacity they write c.c. I thought they were County Councillors when I saw it first. Thousand kilos., is not used —a new name, a tonne.

Sir Fredk. Bramwell.

May I ask your indulgence for another ·0833′ of an hour, or better, as an hour is not a decimal portion of a day, for another ·00347 of a day?

It is very good of you to accord it to me, but I question how many of you knew that I asked for five minutes.

I will use these five minutes to call your attention to the sacrifice we should make of the short cuts that we have come to from the practice of years. I am too old a dog to learn new tricks; it would take me some time to find similar short cuts with the metric system. These are some of the short cuts with our present weights and measures:—A foot super of iron, $\frac{1}{8}$ in. thick equals 5 lbs. Cubic inches of cast iron, divide by 4 and add $\frac{1}{20}$th you get lbs. In round iron you only want to know the diameter in eighths and you get the weight in feet immediately. Take for example $1\frac{3}{8}$. That is $\frac{11}{8}$ths. Square $11 = 121$, divide by 25, or multiply by ·4, equals 4·84 lbs. per foot. That is a short cut in which you may usefully employ a decimal. One cubic foot of water $= 62\frac{1}{2}$ lbs. = 1,000 ounces. One inch deep of water off an acre is 101 tons; an inch off a square mile in a year is 40,000 gallons per diem. Every yard stroke made by a pump gives you a lb. for each circular inch. Thus an 8 in. pump, 8 times 8, 64, 6·4 gallons for each yard. I use the ·4 when I want to. A halfpenny laid on an inch ordnance map covers 500 acres. Nothing has been said by Mr. Siemens about the multiplicity of British local weights and measures, a complaint which has been so commonly made by the advocates of the metric system, and therefore as he has not said anything about it I need not say to him in popular language, "You're another." But it is a question which, if it had arisen, I should have asked permission to read extracts from a letter from the *Times* of the 13th of April, 1896, from our member, Mr. Robert K. Gray; it is a pity it should be lost. It is as follows:—

This ordonnance, be it remarked, was issued about forty-four years after the metric and decimal system was devised, and is now fifty-seven years old. One might suppose that these 101 years would have sufficed to make the use of the system general in France, but this is not so. Precious stones are to-day bought and sold in carats; firewood in cordes; milk in pintes; gravel in toises; grain, potatoes, and charcoal in boisseaux; wine in barriques, feuillettes, demi-setiers, and chopines; wood for construction in pieds, pouces, and lignes; beer in canettes and pots; sugar and tea among the poor people is dealt with in livres, demi-livres, &c. Cattle dealing is carried on in pistoles and écus, and not in francs.

* * * * * * *

The jeweller in exercising his trade finds a carat a very serviceable unit, and no doubt the student of exact sciences finds the decimal relations between the units which exist in the metric system a great advantage; but why this jeweller should insist on the students adopting the units the jeweller finds best, or *vice-versa*, I fail to see.

The weight of a truckload of coals can hardly be conveniently expressed in carats, and as the greatest weight mentioned in the metric system is the myriagramme of approximately 220 lb., this system is also unserviceable for dealings in large weights.

I think that will answer any objection that the metric system has not secured uniformity among the traders in France.

One final remark. In the Press and before the public the advocate of change is always at an advantage over the advocate of leaving things

Sir Fredk. Bramwell.

as they are. The advocate of change wants something different, and therefore he cries out for it, and you hear all the noise he makes. People who are content with the present thing do not go about crying out "We are contented," and therefore you do not hear them, although their opinions are there in larger numbers probably than those of the persons who want the change. I do beg of you to bear this in mind when you see it said "that at an enthusiastic meeting So-and-so said so-and-so, and So-and-so said so-and-so," and to remember that you cannot get people who are content with that which exists to go to an enthusiastic meeting; they do not believe it will be of any use, and they say "What is the good of going to hear that nonsense." People do talk that kind of thing.

I wish, Mr. President, to thank you and the members present for the great patience with which you have listened to me.

The President.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Member.

Arthur Moore Thompson.

Associate Members.

John Martin Blair.
Edwin Edwards.
George Henry Gibson.
Archibald John Hedgecock.
Charles Tranter Linney.
Arthur Edwin McKenzie.
Arthur Mills.
Sven Norberg.
Ernest Edward Smeeton.
Adamson George Wild.

Associates.

John Meikle Boutch.
Andrew S. Gray.
Francis Barritt Hills.
Bertram George Kelly.
Max Levinger
Reginald Lacey Lunt.
Joseph Makin.
Ernest Gordon Dewar Mathews.
Thomas Green Richardson.
Stewart Augustus Sillem.
Sidney Lyon Smith.
Norman West.
James W. Wonfor.

Students.

Keith Bradbury Barlow.
Jesse Haigh Baxter.
Henry Morrison Bremner.
Joseph Griffiths Clare.
Wm. Anselm Coates.
Geo. Mather Craig.
Thomas Russell Davidson.
Bernard Palmer Fisher Deane.
Sydney George Frost.
George Rupert Griffin.
Owen Lewis Ilbert.
William Dallas Long Jupp.
Edmund Weston Kay.
Sidney Charles Kefford.
W. H. Lowe.
Frederick Hill Masters.
Joseph Meech.
Walter Reginald Mickelwright.
Stanley Melbourne Mohr.
Harold W. Purle.
Henry Withers Refford.
Harold Wm. Townend Sabine.
Nils Percy Patrik Sandberg.
James Benjamin Spellar.
Alexander Thomson.
Francis W. Wilson.

The Three Hundred and Eighty-seventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, February 5th, 1903, Mr. JAMES SWINBURNE, President, in the chair.

The minutes of the Ordinary General Meeting held on January 22nd, 1903, were by permission taken as read, and signed by the President.

The names of new candidates for election into the Institution were also taken as read, and it was ordered that these names should be suspended.

The following list of transfers was published as having been approved by the Council :—

From the class of Associate Members to that of Members—

Thomas Hesketh.

From the class of Associates to that of Members—

Walter Cullingford Goodchild.	Hugh Lionel Randolph.

From the class of Associates to that of Associate Members—

David Armitage.	Alexander Lindsay.
Frederick Rains Batty.	Jas. Walker Ormiston (Major R.A.)
Arthur Christian Gibbons.	Geo. Ernest Murray Stone.
Archibald John Howard.	Geo. Stamp Taylor.
Wm. Noel York King.	Lancelot Wm. Wild.

From the class of Students to that of Associates—

Benjamin Adair Malcolm Boyce.

Messrs. I. W. Chubb and C. W. Fourniss were appointed scrutineers of the ballot for the election of new members.

Donations to the *Library* were announced as having been received since the last meeting from H. M. Patent Office, and Mr. Charles Bright, Member ; to the *Building Fund* from Messrs. G. W. Bousfield, M. B. Byng, H. C. Channon. E. Fawssett, S. E. Glendenning, R. J. Hatton. H. E. Herring, E. Pink, R. O. Ritchie, J. Shaw, H. D. Symons, J. A. Troughton, G. Walsh ; and to the *Benevolent Fund* from Messrs. G. W. Bousfield and J. G. Wilson, to whom the thanks of the meeting were duly accorded.

The President.

The PRESIDENT : We will now proceed with the adjourned discussion on the Metric System. You will remember that Sir Frederick Bramwell opened for the English system, and he would now like to add a little to what he said on the last occasion.

Sir FREDERICK BRAMWELL F.R.S.: I wish to thank Mr. Siemens for having endeavoured to call my attention to a mistake into which I had fallen. His observations were perfectly pertinent to the occasion. I imagined that those tremendous penalties in the Acts were penalties invented by the advocates of the metric system to get it enforced. He tried to call my attention to the fact they were a mere repetition of the penalties existing in the Acts which enforce the English system. All I can say about it is, that although they sound equally dreadful in both cases, to my mind under the English system there was so little likelihood of punishment that the penalties, dreadful as they were, were not likely to be inflicted: whereas, if the same penalties are enforced in regard to the compulsory introduction of the metric system, infringement and punishment will, I expect, be very great. Having said that much I wish to call your attention to one thing, namely, that this metric system is being forced upon us practically by the French nation. It does not seem to me becoming to such a nation to force the metric system, or anything else relating to numeration, upon others. Just fancy! to the present day they cannot tell you 99 in words without resorting to the childish expedient of four 20's, one 10, and a 9, and those are the people who are going to try to make the whole world go right! Sir Fredk. Bramwell

The only other thing I have to say is to ask Mr. Siemens, when he is consulted about the framing of the new Bill, to put in a "Conscientious Objector" clause, as they did in the Vaccination Act.

Mr. J. EMERSON DOWSON: I think that in a serious discussion such as this is, it is important that we should have clearly in our minds why we are comparing the metric weights and measures with those of the Imperial system which is now in use. I hope you will agree with me that it is not merely to contrast their relative merits, but, above all, to see how far we can and should adopt an International system—a universal language, so to speak, of weights and measures. With these few preliminary remarks I will, not only for myself, but for the Decimal Association whom I represent, endeavour to meet some of the points which Sir Frederick Bramwell has brought before us. Mr. Dowson.

In the first place, with regard to Napoleon. No one can question the military genius of the great soldier-emperor, and he may have been an authority in his time on the subject of weights and measures; but whether he was so or not, I can hardly suppose that our engineers and manufacturers of to-day will be much influenced by what Napoleon may have said on the subject some eighty years ago. Since then our commercial relations with other countries have changed greatly, and whether we like it or not, the plain fact remains that after France had proved that the metric system of weights and measures was practical and good, it was adopted by Austria, Belgium, Germany, Greece, Holland, Italy, Norway and Sweden, Portugal, Spain, and Switzerland; in fact, by all the European countries except Great Britain and Russia. It has also been adopted throughout South America and in Japan. As regards Mr. Beresford-Hope, Sir Frederick Bramwell advised us all to read the speech made by this Member of Parliament some forty years ago against the metric system, and I for one have done so. He said:

r. Dowson. "Only imagine an honest housewife going into a shop and asking for a decigramme of pepper, and a dekagramme of tea." I reply that in the ordinary affairs of life no one wants to buy the tenth part of a gramme of pepper, or of any other commodity. Also the term dekagramme is seldom if ever used, and the purchaser should ask for ten grammes and not a dekagramme of whatever he wants. Mr. Beresford-Hope also said: "If a housewife has to cut up a loaf for her family she divides it into 2, into 4, into 8, or into 16 parts." I don't think this is the usual way of dealing with the domestic loaf, but, after all, what has it to do with the practical adoption of a decimal system for general purposes, seeing that the binary divisions will always be used, and should be used as far as possible, whatever may be the system of weights and measures. Mr. Beresford-Hope, however, went further, and said: "Supposing the loaf to weigh originally a pound, each of these sixteen divisions comes to an ounce." But one might reply that, supposing the housewife had to divide the loaf into five or ten parts of equal weight, she could not do so with the ordinary household weights of pounds and ounces, whereas in the metric system it would be quite easy. The fact is that no system is the best under all circumstances, and from a practical point of view we have to select that which presents the greatest number of advantages. As to foreign trade, Sir Frederick Bramwell admits that our manufacturers and merchants must be handicapped in their dealings with metric countries, if they do not make and sell on the metric basis. But to do this under existing conditions they must use one set of weights and measures for their foreign trade and another for the home trade. There is not time for me to consider all the losses and inconveniences this entails, but I think you will agree with me that a dual system has many obvious drawbacks, and cannot be right. So far as I can judge, the remedy is to have one international system for all purposes.

Decimals.—The importance of always dividing without a remainder has been greatly overrated, and actually all scientific men of the present day use the metric weights and measures and no others. It is also a mistake to suppose that if a decimal system were generally adopted, vulgar fractions could not, or would not, be used. Decimals are not upheld as exclusively right, but rather as preeminently convenient; and when in certain cases it is quicker or more accurate to use vulgar fractions, undoubtedly they should be used. This is the common practice in all metric countries.

It is also worth noting that at the present day a *decimal* system of coinage has been adopted by *every civilised country in the world*, except Great Britain and some of her Dependencies. It is, moreover, a fact that in no single instance has any country given up the decimal system after once adopting it.

Decimal Point.—On architectural and engineering drawings it is now usual to write all dimensions under one metre in millimetres, *as whole numbers*, without any decimal point. They are then added together to get the main dimensions, and, if the total exceeds one metre, the metres and millimetres are separated by a mere stroke of *the pen.* Compared with feet and inches, and fractions of inches, the

operation is not only more simple, but there is less risk of mistakes being made. Mr. Dowson.

In makers' price lists of pipes, steam fittings, etc., it is usual to give all the dimensions in millimetres only, without a single decimal point.

In this country a dot is generally used to denote the decimal point, but on the Continent a comma is used, and I think the latter is safer, as it is larger and more conspicuous.

Nomenclature.—I agree with Sir Frederick Bramwell that a long list of composite Greek and Latin names is undesirable, but in actual practice only a few are used, even in France, where the system originated. We may take Germany as representing practical up-to-date teaching, and in the primary schools of that country it is usual to teach the units and sub-multiples, and only a few multiples. For all practical purposes we need only the metre, divided into centimetres and millimetres; the litre divided into decilitres; and the kilogramme divided into 1,000 grammes. There is really no need of any tables. The metre can be squared for measures of surface, or cubed for measures of bulk or volume, just as yards or feet are squared or cubed.

If preferred, the word *pound* can be used for half a kilogramme, just as the *livre* is used in France. The mere name is comparatively unimportant provided the *value* is understood in grammes. The names centimetre and millimetre are no more difficult to understand or remember than such words as subtraction, multiplication, telegram, etc.

[*Communicated after the meeting:—Relations of Units.*—Sir F. Bramwell said little about these, but they are important features of the metric system, and are of great value. We know, for instance, that one cubic centimetre of water, at its maximum density, weighs one gramme; that 1,000 cubic centimetres, or one litre, of water weigh 1,000 grammes, or one kilogramme. Hence one cubic metre of water contains 1,000 litres, and weighs 1,000 kilogrammes, or one (metric) ton.

As the density or specific gravity of a substance is its weight in relation to that of water, and as the weight of water contained in any metric measure of volume is known, it follows that the weight of any substance can easily be found when its bulk in terms of the metre is known. We have only to multiply the volume or bulk of the substance in question into its specific gravity, and we shall know its weight. Or, conversely, if we know the weight of any substance, we can easily find its volume by dividing the weight by the specific gravity.

It is certainly a gain to have a uniform *volume-unit* of water, as in the metric system; for all calculations of volume and weight of other substances are then simple and rational. In our present system the volume-unit is sometimes a gallon, sometimes a cubic foot, and sometimes a cubic inch, and there is no uniform basis to assist the mind in comparing various substances.

Mental Arithmetic.—Sir F. Bramwell has very properly urged the desirability of practising mental arithmetic, but thinks that with the metric system this cannot be done. I venture to say that if he will be good enough to look over some of the French and German books of

r. Dowson. arithmetic of to-day, and will ascertain what is actually done in the primary schools of those countries, he will find that attention is given to this, and that mental gymnastics not only are possible, but are practised, with the metric system.

Short Cuts.—I can readily understand that after his long and varied experience, Sir F. Bramwell is loth to give up the short cuts and *aides mémoire,* which he has found it necessary or useful to adopt in connection with our present weights and measures. With the metric system short cuts are seldom, if ever, needed, and as Sir Henry Roscoe once remarked on this subject, "We do not want a short cut if the road is straight."[1]]

There is much more I could say on this interesting subject of weights and measures, but time forbids. In conclusion I would remind you that the conditions of life in all civilised countries have altered greatly during the last quarter of a century. Each country has developed its resources and manufacturing powers enormously; steam and electricity have done wonders; there are much greater facilities for travelling; and there is much greater intercourse between nations. This will not diminish, it will increase; and to save our time, and to help us in our dealings with foreigners, surely it is worth while to put up with some temporary trouble and inconvenience, to rid ourselves of our present complicated weights and measures, and to adopt those which have been adopted irrevocably in nearly all civilised countries.

Let us remember also that we have to think of our Colonies and Dependencies. As a matter of fact most of them are really anxious to adopt the metric system, and are now looking to the Mother Country to take the lead. This is no mere opinion on my part. The subject was discussed by the Colonial Premiers at their Conference in London last year, and they passed the following Resolution:—

> "That it is advisable to adopt the metric system of weights and measures for use within the Empire, and the Prime Ministers urge the Governments represented at this Conference to give consideration to the question of its early adoption."

Actually we are within measurable distance of getting an international system for all purposes if we will but have it. Men of science have adopted it already, and I ask you, Why should not engineers and manufacturers do the same?

Mr. Parker. Mr. T. PARKER: Most of what we hear is usually aimed at the metric system; we do not find criticism of the system. When we look at the metric system as a whole we think of two quantities. The metric system of weights and measures consists, shortly, of a volume measure based upon the measurement of length, and of a weight unit which is the weight of that volume of water, the unit of specific gravity and the thermal unit being based on certain properties of water. We thus have a connection between the mechanical, electrical, and physical units, and so science has in this system a sort of handmaiden without which it is impossible to proceed. We cannot remain

[1] The Communicated portion of Mr. Dowson's remarks ends here.—ED.

Mr. Parker.

TABLE I.

Relative Dimensions of the Metre and Derived Units.

Metre Unit.	Cubic Metre.	Metre Ton.
Decimetre $= \frac{1}{10}$	Litre $= \frac{1}{1,000}$	Kilogram $= \frac{1}{1,000}$
Centimetre $= \frac{1}{100}$	Millilitre or Cubic Centimetre $= \frac{1}{1,000,000}$	Gram $= \frac{1}{1,000,000}$
Millimetre $= \frac{1}{1,000}$	Cubic Millimetre $= \frac{1}{1,000,000,000}$	Milligram $= \frac{1}{1,000,000,000}$

Note.—$\frac{1}{2}$ Millimetre = nearly $\frac{1}{50}$ inch; $\frac{1}{4}$ Millimetre = nearly $\frac{1}{100}$ inch; $\frac{1}{10}$ Millimetre = nearly $\frac{1}{250}$ inch.

TABLE II.

Inch Units.

Inch Unit.	Cubic Inch.	Cubic Inch of Water Weight.
Mill. $= \frac{1}{1,000}$	Mill. Volume $= \frac{1}{3}$ Minim $= \frac{1}{1,000}$	Mill. Weight $= \frac{1}{4}$ Grain $= \frac{1}{1,000}$

NOTE.

960,000	Mill.	= 80 feet = 960 inches.
3·6	Mill. volume	= 1 minim.
1	Mill. weight	= ·252 grain.
62,000	Weight units	= 1 ton.
25	"	= ·9 lb.
25	Cubic inches	= ·09 gallon
10,000	" "	= 36 "
73,000	Inches	= 1 sea mile.
61	Mill. volumes	= 1 cubic centimetre.
61	Cubic inches	= 1 litre.
61	Mill. weights	= 1 gram.
61	Inch weights	= 1 kilogram.
62,000	Inch weights	= 1 ton.

Government Survey—

25 inches	= 1 mile.
1 square inch	= 1 acre.

The ratio of inch units to metre units of weight, or volume, is 5 (inch) units are equal to 82 grammes or cubic centimetres correct to $\frac{1}{1,30}$.

$$50^3 = 2{\cdot}0125 \text{ tons.}$$
$$25^3 = {\cdot}2514 \text{ tons } (\tfrac{1}{4} \text{ ton} + \tfrac{1}{160}).$$

Parker. where we are. Now, if we go on we must, according to the present verdict, accept the metric system in its entirety.

After using the metric system for thirty years, and after having devoted some four years of critical study to it, I say that the units of that system are entirely unsatisfactory, and imperfect. I have carefully read the whole of the information given before the American Commission; I have heard what Sir Frederick Bramwell has said, and I have read the Commission's reports in this country, and all else I could find that has been written upon the subject. I have discovered no objection made to the metric system of weights and measures, but I have found the objections to apply wholly to the dimensions and ratios of the units used with the metric system. As Sir Frederick Bramwell very wisely pointed out, we are bound to decimals by the units of the metric system. The form in which the question presented itself to my mind is set out in Table I. (p. 301). Those dimensions of the units of the metric system mean the entire destruction of the whole of the measuring systems of the world, excepting that of those who introduced it, and they had to introduce it to the destruction of their own. What do we find in looking over the evidence of America? We find men coming forward, representing large manufacturers in the country, saying that they do not want it; we find such men as the Chief Constructor of the Navy of America saying, "We do not want it." But they did not go into the matter and see if it could be improved. The only way in which you can improve the units of the metric system is by altering the length of the metre, and they did not do that for Whitworth, nor will they do so for us to-day. But you cannot alter the units so as to retain the metre; they are fixed by the length of the metre; you have to put up with them in their present form. They are a cumbersome lot; they begin by initiating fractions, fractions of fractions, cubes of fractions, $\frac{1}{3}$ and $\frac{1}{4}$; and even then you do not get what we have in our ordinary inch and 1,000th. I was led to make a number of exploiting figures and experiments, to try to find out where we could amend the metric unit, and came to the conclusion, that if we are to have a perfect set of units with the metric system there is only one length that can give it, and that is the inch length, and there is no improvement possible upon that. I have set that out on Table No. II., and you can compare it with Table I. You will see that we want but one set of units. I have put below the inch units, the fraction of a 1,000—namely, the mill., the mill. weight, and mill. volume. You will see that those units are perfectly equal whether you multiply them or divide them to any extent: they always remain in ratio, and they do not confine you in any way to decimal arithmetic. You can employ all the arithmetic you are now using, and you may pass to the metric system if you permit the inch to be used as a unit to-morrow, and you will then have all the advantages of that system. You can improve the metric system, and you can improve the new metre units off the face of the earth, by simply sanctioning the use of the inch as a unit of length, and legalising the weight of a cubic inch of water as a unit of weight, and meet no objection. Four or five years ago I should have been as willing as anybody to crush the measures of England for the supposed

Mr. Parker.

benefit of the metric system; but to-day I say that if you do so you will disgrace yourselves for all time. You are all asked now to adopt the metric system in your country, and you are going to do it at the expense of a social revolution, and after an expenditure of millions of money, and in the end you will only get an imperfect thing whilst you could have had a perfect thing. Here it is already established: simply legalise the weight of an inch of water, permit the use of the inch as a primary unit of the metric system, and you have all that you can get by the metre and a great deal more. You simplify the units, and make them perfect, and you disturb nothing. I do not advocate suppressing the metre; but from what we hear of our American friends—I hope there are some of them here—they always scrap anything when they find something better, and I shall expect that they will at once scrap the metre and all that belongs to it, and adopt the inch.

I do not want to trouble you with history—we have had enough already—nor with cross-calculations, because we know all about them. I will try to inform those gentlemen in the House of Commons who will be called upon to decide for this country what they will do in the matter of weights and measures, as to the virtues of the inch, and then I shall be conscience-clear. They are men who will, I trust, look after the economic policy of the question, and we shall be permitted to use our venerable old inch in its proper place and character, and get a simple and perfect set of units for the metric system. It is impossible to enforce the metre. It would be a calamity to do so.

Sir Andrew Noble.

Sir ANDREW NOBLE, F.R.S.: I find myself placed in rather a peculiar position, for I find myself opposed to a man for whom I have the greatest reverence—I mean my friend Sir Frederick Bramwell. But we agree upon so very many points that I think we may agree to differ upon this one point, or rather I should say two, for the metric system is mixed up with the decimal system: both are involved, and I think the decimal system is even more attacked than the metric. As regards the decimal system, I must say I think it a scandal to this country that we have never had our pound sterling (£) decimalised. At present every silver coin we have is an exact decimal of a pound: the only thing we want is the alteration of making a shilling fifty farthings instead of forty-eight. In that case every subdivision of a pound would be represented by three decimals, whereas at present, if you get 19s. 11¾d., you have six figures. The difference in rapidity of calculation is enormous, to say nothing of the power of using very simple calculating machines. I mention this as a point was made in regard to reducing the number of figures.

I must say, having had a great deal to do with fo[illegible]ries, I find a very great inconvenience in our English syst[illegible]nd measures of all sorts. Some of our measures, [illegible] thermometer, are founded upon errors, and our w[illegible] haphazard. Taking our weights, the only con[illegible] of is the grain. Our ounces are different, [illegible] I daresay all of you have heard the qu[illegible] a pound of gold or a pound of feath[illegible]

r Andrew oble.

whole of the possible answers given to the question. The difficulties that are placed in the way of any one, who, like myself, has to compare large numbers of foreign weights and measures are almost insuperable. I recollect the late Professor Hoffman saying to me once when pointing out the inconvenience of our measures, that if you wanted to make any considerable induction, the observations of philosophers or men of science in one country were a sealed book to those in another country. Since he said that, there has been a great advance in the use of the metric system, and I need not say that almost every scientific society, with which I am acquainted, refuses to receive papers that do not use the metric system.

The metric system is, in my opinion, a most carefully thought out and perfect one. It is based upon one single element—that of length. The metre is taken, I think, at the 10,000,000th part of the quadrant of the great circle of Longitude passing through Paris. It is prolonged by decimal multiplication, 10, 100, 1,000, etc., and for these are used the Greek prefix. For subdivision they use the Latin. If you take the smallest weight, the gramme, that is the weight of water in a centimetre cube ; the kilogramme is a decimetre cube, and the ton is the metre cube. Observe that it has these great advantages. You know the weight of a ton is a metre cube of water. If you know the specific gravity of any metal or other material you are able at once to give the weights in tons and decimal parts. Then the metre and its subdivisions satisfy the superficial measurement, and the one great advantage the metric system has, is that if we adopt it we have with that a uniformity of measures for all countries. I need not say it is hopeless to get other nations to adopt our system, and nobody can dispute the advantage it would be to us if there was but one system of weights and measures throughout the world. I will not recall to your minds the number of nations that have adopted the metric system, but I will say that in my opinion the metric system ought to be made compulsory within a certain number of years. The question is how to do that with the least inconvenience.

A most admirable proposition was made by my friend Professor Johnstone Stoney, whereby we might alter our weights and measures almost infinitesimally, and, at the same time, keep for those who prefer it the same nomenclature. Professor Johnstone Stoney in his paper proposed that the new yard should be exactly 9 decimetres. Most of us I daresay when, for rough calculations, we consider the centimetre, call it 0·4 in., and the inch 2·5 c.m. The alteration is very slight in making the inch exactly 2·5 c.m., and in that case the yard would be exactly ·9 of a metre and would only be altered by an exceedingly small fraction, about 1 per cent. of its length : the same would apply to the foot and the inch. You might keep for a certain number of years, or altogether if you like, the old nomenclature. The inch, as you know, would then be 25 millimetres ; all of you have seen an inch divided into 100 ; it would only mean that the inch would be altered by 100th of its length, and there could be no great inconvenience in that. Then I think Professor Johnstone Stoney proposed that the mile should be 1,600 metres. That means that our mile would differ from our present

measure by only about the breadth of a narrow lane, and no inconvenience would result. In regard to weights, the new pound (lb.) would be 4½ hektogrammes exactly, which would differ from our present pound by under 1 per cent. If these systems were adopted, they might prevent the inconvenience to which some of the speakers have alluded, that is to say, there would be no rough break in our ideas, and I do not think the public would suffer much. Anyhow, I desire, in conclusion, to express my opinion that sooner or later the metric system must be adopted, and I would add that I think it ought to be as soon as possible, and that the change should be effected with as little inconvenience to the public as possible. Sir Andrew Noble.

Mr. LESLIE S. ROBERTSON: I did not intend to speak to-night for more reasons than one, but as you have called upon me I will add a word or two on the practical side of the question. It has been my privilege to superintend work in countries where the metric system is in use, and also in this country. I had, after following very carefully the construction of torpedo boats in this country in a torpedo-boat works not very far from London, to go over to France and build similar boats and boilers for the French Government in one of the leading works in France. One of the difficulties we had to encounter was this. An English piston-rod would be to some even dimension, say 2 inches, but when we got abroad nobody would think of making the piston-rod 50·8 mm. Therefore the whole of the drawings had to be redrawn, and what was our 2-in. rod here was not our 2-in. rod there. Then we had another very serious inconvenience, namely, in connection with the screw threads. Our British threads are stronger and deeper than the metric threads; and when certain classes of work are arranged for British threads they do not come in comfortably, to say the least of it, on the metric system. It is felt in France and elsewhere, I think, too, that the metric threads are not altogether satisfactory. This was emphasised by another practical experience. When I was abroad last year with one of the Government Commissions studying the question of locomotives, I made careful inquiries and found that, in the most up-to-date German works, in fact, practically in every works that we visited, they were using our Whitworth thread and not the metric thread. Mr. Robertson.

I do not know if there is any definite proposition before the meeting. We all admit the advantages of the metric system, and they are many, but it is no good merely discussing the question from a general point of view. The metric system is permissible in this country; anybody can use it who wants to. The question is whether this meeting thinks it justifiable to urge upon the Government, which is the only body that can take any action in the matter, that they should make the metric system compulsory. That is a very drastic step to take. Perhaps there will be some definite resolution before the meeting prior to its close, but I venture to think that the meeting will have to consider very carefully before they come to a vote on the question as to whether they are in favour of making the metric system compulsory in this country. It is now legal, as I said before, and anybody can use it who wishes to do so, but it is a very open

Mr. Robertson. question whether public opinion is sufficiently ripe for the metric system to be compulsorily forced on the country by the Government.

Dr. Stoney. Dr. JOHNSTONE STONEY, F.R.S.: Sir Andrew Noble has already spoken of the system I have proposed, and I do not think it necessary for me to enter on any of the points that he has referred to. I think myself justified in claiming that Sir Andrew Noble's opinion ought to carry special weight, both with the people of England and in Parliament, on account of his unexcelled experience and acquaintance with every aspect of the question in debate, and because the opinion he has expressed here to-night is an opinion formed with great deliberation. He has here reiterated, after two and a half years' further consideration, the same judgment as he expressed in 1900, when my proposal was first put forward. Our present position in reference to weights and measures is simply this, that by recent legislation we have been relieved from being publicly punished, if we buy or sell by metric weights. There have been three Metric Acts, one the Act of 1864, which was intended to render the system permissible, but which failed in that object in consequence of the legal difficulties that were raised. So matters remained for a number of years, till 1878, when for the second time an Act was passed dealing with the use of metric weights in this country. It supplied the omissions in the Act of 1864, but while permitting the use of metric measures for other purposes, it forbade their use in buying and selling. The result of that state of things was, that metric weights came into use for all scientific purposes in this country, but they could not be used by any chemist who kept a shop, and the Act compelled chemists and druggists to continue the use of the bad system known as Apothecaries weights and measures. In 1896 the Government introduced a Bill to get rid of the restriction introduced into the Act of 1878, and they appended to the Bill they introduced in that year a body of equivalents similar to, and equally cumbersome with, those under the Acts of 1864 and 1878. The Bill was withdrawn in that session, but in the next session it was passed without the objectionable tables, and improved tables were, in 1898, issued by an Order in Council. These are now the tables of equivalents that have legal force in this country.

Since then deputations have waited upon the Government; and both Mr. Balfour and Mr. Ritchie in answer to these deputations said that the next step to be taken was by the people, and not by Parliament. They were under the impression, as the last speaker was, that the late Bill did all that was necessary to render it permissible to use metric weights and measures. All that the Bill does is that it prevents a person who uses them from being publicly punished. That is not sufficient. It is necessary to make it possible for them to use them without incurring large trade losses. The deputation recommended that the metric system should be introduced compulsorily within two years. My own impression is that the duty of our governors is rather to lead the people than to drive them, and I submitted the proposal referred to by Sir Andrew Noble, suggesting that the best legislation to introduce would be to alter our standards of length and weight without altering the relations in which the different sub-divisions of the

Imperial system stand to one another—altering them by excessively small quantities, which will suffice to bring them into simple relation with the metric system. If this proposal were carried out it would be possible for work to be begun in one shop where Imperial measures are used, and continued in another shop where metrical measures are used, and nearly all the other practical difficulties which prevent business men from adopting metric measures would be got over. I am myself persuaded that we may wait for fifty years if the only policy suggested to Parliament is to pass a Bill making the use of Imperial measures illegal. I would claim that persons occupying the attitude of Sir Frederick Bramwell should accept with readiness my proposal. He has expressed his desire that the two systems should be tried, leaving whichever is the better to win the day; and the way to do that is to bring them into such relation as to make the use of either system possible. All the advantages which he supposes attach to the existing system would be preserved if this proposal were carried out; and we might all look to the future with confidence, including those of us who hold, as I do and as Sir John Herschel did, that the people of England, if enabled practically to try both systems, will prefer that one of which the divisions are brought into the most natural relation to our system of numeration, and of which the weights and measures of capacity are connected in a rational way with the measures of length. Dr. Stoney.

The PRESIDENT: I will now read a letter that I have received from Lord Kelvin:— The President.

Lord KELVIN (*communicated*): Will you tell the meeting to-morrow that I am very sorry I cannot be present to hear Sir Frederick Bramwell, and to endeavour to convince him that the universal adoption of the French metrical system by electrical engineers and engineers of all classes, and common-sense people throughout the country, and in all saleshops and workshops and factories, will be a great blessing to every individual person concerned, including home and colonial British subjects and the whole rest of the world. Lord Kelvin.

Sir WILLIAM PREECE, F.R.S.: On this particular subject the line that I take up is this. It is no new fad of mine, for I find that in March, 1853, exactly fifty years ago, I wrote a paper on the advantages of the decimal and the metrical system, and my fifty years' experience has been an addition to the strength of the feeling I had then and have now, that there is a great deal in the metrical system, but a great deal more in the decimal system. The two are totally different: you must deal with each on its own merits. First of all, I say that if the metric system or the decimal system is to be introduced, it must be because there is a necessity for it. Either there is a necessity for it or there is not. If there is not, no compulsory Act of Parliament, no resolution of this Institution will force on this country a thing that is not wanted. If it is wanted it does not require an Act of Parliament to enforce it. Necessity, commerce, the demands of trade, the business of this country, will introduce a system, decimal or metrical, into the habits and customs of our workshops. Sir William Preece.

Our system of weights and measures is, in spite of Sir Frederick *Bramwell, execrable.* *Why,* gentlemen, there are 154 distinct units of

r William reece.

length to be found in different parts of this country. I would like to read the list through, if I had it: it is in an appendix to a paper published by the Society of Arts. But think of 154 units of length: there are the hand, the span, the cubit, the foot, the yard, the ell—I could go on for ten minutes if you would allow me—but I will stop! We only want one real, good unit of length; in the same way we only want one good unit of mass or weight, and of volume, and everything that is required. Mr. Parker has started a new hare. He has based a system on the inch, and he says the inch cannot be improved. To a certain extent I agree with him, but I go further, and I say, "Cannot be made worse." I asked him just now, What is an inch? He said he had gone back to the period of the Saxon kings, and he found that Edgar had introduced the inch. [Mr. PARKER: Yes, he kept it at Winchester.] Let him go a little further, and what will he find? He will find that the inch is three barley-corns in a row, picked from the centre of the ear of corn—and that is our British inch! Now if that inch were a scientific inch, it would not be a bad system of units. Take Professor Johnstone Stoney's inch: it is much better than three barley-corns.

I want to urge that, in the first instance, there is no use attempting to change our units unless there is a necessity for it. In the second place, if there is a necessity for it, and I believe there is, the whole trade of the world involves the use of a uniform system of units, and that system must be, with our present knowledge, based on the metric system. Now with regard to the use of the metrical system. We hear from Sir Andrew Noble that it is introduced in the Elswick Works. I know from personal experience that it has been used at Willans & Robinson at Rugby, it is used by Siemens, it is used by Greenwood & Batley at Leeds, it is used by the Shropshire Iron Company at Wellington, and indeed I do not know by how many other places. Is there any single electrical engineer in this room who does not use the metrical system? I have been a member of the Committee of Standards of the British Association for over a quarter of a century, and we have introduced the c.g.s. system of units, which is purely metrical. Is there a man in this room who has found any difficulty in applying it? We have had twice to change the value of the ohm: has it caused any difficulty or trouble? On the contrary, throughout the whole world, in every country without exception, the metrical system has been universally employed. I am glad to see that even my friend Mr. Parker, with his three barley-corns, has gone so far as to meet the metrical system half-way by introducing what, after all, is the half-mile post to the ultimate goal, the decimal system. I say if we make any change at all, it must be the whole metrical system. I do not say there is a necessity for it, but I believe there is; and I do say that the time is not very far distant when we shall be forced by our trade and by our commerce to adopt either the metrical system in its entirety, or a very close approach to it.

:. Brough

Mr. BENNETT H. BROUGH: I doubt whether Mr. Siemens was right in ascribing to James Watt the fundamental idea on which the metric system is based. Watt's ingenious idea of dividing the pound decimally was anticipated in 1620, by Edmund Gunter, who proposed a decimal

measure for land, the unit being the chain of 100 links. This convenient measure by which 10 square chains were made equal to an acre, is still in general use, and the conditions of land tenure are such as to render its displacement unlikely. The engineer for levelling uses the foot decimally divided. It is evident, therefore, that he is not averse, when necessary, to using decimal measures. The matter is entirely different if he is to be publically punished by fine or imprisonment if he uses anything else. The whole question is one of commercial expediency. It is quite possible that to the electrical engineer the compulsory change, which would undoubtedly be of inestimable advantage to Germany, would not be so disastrous as to the mechanical engineer, for the electrical engineer derives great benefit from German skill. Customs returns show that of electrical machinery the British imports from Germany are twenty times as great as the British exports to that empire. In other branches of engineering the change would be all to the advantage of Germany. The cost of compulsory change would be enormous. Every retail trader would require a new set of weights and measures (which German makers would have in stock to supply). Every gas pipe would have to be torn out of our houses. The Whitworth thread would have to go, all gauges and templates scrapped, and the working man fined or imprisoned if he drank his beer out of a pint pot. Even in Germany the change had been made with difficulty; and Rhenish inches, Lachter, Thalers and Groschen are still found in use. Mr. Brough.

In discussions of this kind, the metric advocates are apt to be led astray by their enthusiasm for a fantastic natural unit, which after all, as the meridian was inaccurately measured, is only an arbitrary one. Thus the author quoted by Sir W. Preece, who gave a long list of obsolete measures with a view to discrediting the Imperial system, omitted to mention that by the Act of 1878 the user of any of these measures was liable to a fine of £5, or of £10 for the second offence. Teachers of arithmetic are much to blame for the existing confusion, and for the retention of illegal measures. In the latest book on arithmetic, just published by the Cambridge University Press, the scholar is taught that in civilised countries, England being unfortunately an exception, coinage, weights and measures are arranged on a decimal plan. The author does not tell us that Great Britain, the British Colonies and Dependencies, the United States and Russia, where the metric system is not used, represent 40 per cent. of the world's population. It would be ideal if all nations spoke Volapük and used the metric system. But it is doubtful whether the uniformity would compensate for a change that would forbid the use of division by continued bisection, destroy our standards, and render our technical literature useless.

Sir John Wolfe Barry.

Sir JOHN WOLFE BARRY, F.R.S.: In rising to say a word in criticism, both of the metrical system and decimal system, I feel that one is somewhat like the man who was accused of speaking disrespectfully of the equator, because really one has heard so much said, and so weightily said, in favour of the metrical and the decimal system, that it requires some courage to raise one's voice on the other side of the question. I am bold enough however to say that I am averse to the

Sir John Wolfe Barry.

metre as a unit, and that I am averse to the decimal system for many purposes of calculation. I do not mean to say that both may not be useful to many people, but I am perfectly certain that they are by no means desirable for everybody. I do not wish for one moment to defend the system of weights and measures in this country. They are complicated and unscientific, there is no doubt, but that has nothing to do with the question of adopting either the decimal system or the metrical system. Mr. Brough has said that the proportion of people who do not use the metrical or decimal system is 40 per cent. of the whole of the population of the globe. I have been informed, but I will not vouch for its truth, that they are in excess of half. [Mr. ALEX. SIEMENS: With the Chinese ?] No, not the Chinese—we do not really know what system they use in China; so we will leave China out of the question. But when we are told that we must of necessity come to the metrical system because the Latin nations, helped up by Germany, have adopted it, I would like to recall the time when we were similarly told that everybody must learn French or they would not be able to communicate with people on the Continent. What has been the result? English is far more universally used than any other language in the world. Our object ought to be to get the best system, and to see that we are satisfied that it is the best system. If it be the best system, I hold that Great Britain, with her Colonies and Dependencies and the United States, will set the tune, and that other nations will follow. Therefore, let us see what is the best, and not rush at the metrical or decimal system, because we are told that a number of people who, with the exception of their latest proselyte, Germany, are not great commercial peoples, have adopted the metrical system.

I have worked in times past a good deal with my hands, and I am certain that the metre is too large a unit; it is not convenient. The foot is infinitely more convenient for all work in which I was engaged. Again I hold most strongly that the possibilities of division of 12 are far more convenient for practical men than any system based on 10. I spent also a great deal of my time once in what is called quantity measurements, where we are continually dealing with complicated fractions —2 ft. 4 in., 12 ft. 8½ in., 5 ft. 11 in., and all that description of dimensions, both in computing areas and cubes; and I know that the fact that the foot is divided into 12, makes rapid computation far more easy than any division into 10.

When you can divide your unit, first of all by 2, then by 3, then by 4, then by 6, and then by 8 for 1½ in., I hold that a duodecimal system of measurement and computation is far and away more convenient for rapid practical work than any decimal system.

We talk about international trade. What is our position in regard to international trade, and how much do we export into all these protected countries who have adopted the metrical system? Why do we want to make it easy for continental nations to supply our home and colonial markets when we get no reciprocity whatever? I am not saying that that is a reason in itself against the decimal system or the metrical system, or in favour of the duodecimal system, but I merely allude to it because those who so ardently advocate the metrical and the decimal

system say that it is necessary for our international trade. I do not think it is necessary for international trade in the sense in which that is understood. Some previous speaker said with great truth that it will make it uncommonly easy for our friends who are represented by Mr. Siemens here, or who are at any rate represented by his family, to compete with us in every market in the world. I have no doubt it may do so, but is that a reason for our getting rid of convenient units and adopting inconvenient units? Sir John Wolfe Barry

There is another point on which I wish to touch. What is to be done with the measures of time? Is the day to be divided into 20 hours instead of 24 hours? What is to be done with all the measurements of latitude and longitude? Are they to be all made decimal, and are our sailors to be forbidden to sound in fathoms?

But what is the reason for all this advocacy? And why make it compulsory, for that is what is really intended by its advocates? In 1895 it was made permissible; let those who want to use the metrical system or decimal system continue to use the metrical or decimal system, but it does seem to me perfectly ridiculous, in this year 1903, to think for one moment that Parliament would compulsorily enact the use of weights and measures which would be inconvenient to a very large proportion of the population and hugely costly. Why not, as Lord Melbourne said, leave it alone? Those people who have to supply countries which use the metrical system with girders and similar articles can use the metrical system. What more do they want? Both systems are useful, I readily admit that for scientific purposes; and very likely for electrical purposes, with which I am not so intimately acquainted, it may be exceedingly useful to use the metrical and decimal system. But why make it compulsory? We can get on very well as we are. I do not, however, suppose that a meeting like this is going to pass any resolution, because that is not our business. We are here simply for the purpose of discussing the question and exchanging ideas, and I should deprecate any other aspect of the subject.

We are told that by some very elaborate system we could in a state of transition make our present units fit the metrical units. That means that we shall have three systems at work. Can that be useful or practical?

I venture, in conclusion, to say that first of all any system which is divided by twelve is the most convenient thing for the working man. I believe I am right in saying that to this day in Paris, the very heart of a compulsory metrical system, the opticians' work is done by the old French inch, which is divided by twelve, and that the dozen is still used in France in many trades. I do not speak from want of experience in practical work, as I worked at the bench myself for a year and a half, and I have spent a great deal of time in the computation of measurements. I accordingly put before this meeting my strong opinion that the duodecimal system has advantages to which the decimal system can never attain, and that if you compulsorily adopt a decimal system you will absolutely destroy that mental arithmetic with which most of the people who have to do with measurements make with ease and rapidity almost all their more simple calculations. Therefore, as I said

Sir John Wolfe Barry

before, although I feel that perhaps one is speaking to many people who are convinced the other way, I raise my voice against any compulsory adoption of either the metrical or decimal system, although I firmly believe that both are useful in their proper places.

Mr. Tannett Walker.

Mr. F. W. TANNETT WALKER: I have come over two hundred miles for the purpose of speaking because I am a very strong believer and thinker on the subject. First of all, a word or two about the money. I am a good deal associated with Chambers of Commerce, and these Chambers of Commerce are almost universally very strong supporters of the decimal system. I am not. They do not seem to understand what they want, and none of them can agree as to what their money standard is to be. One gentleman proposed a half-sovereign, another gentleman suggested that it should be called a Victoria, and another recommended a florin. They recommend all sorts of computations, but none of them can agree. As long as they cannot agree, I think it is absolutely fatal for the good of the country that they should attempt to change the standard. People who cannot agree have no right to ask for change. England has many good assets. We have a very good Navy, a very good Army, and a very good reputation; and as far as I am concerned I believe that the best part of our reputation is due to the constancy and never-failing value of the sovereign. If any of you have known what it is to send your family into Germany and then have to pay the bill, you will find that when you get the bill for your three weeks' holiday and give them an Englishman's cheque—not in sovereigns—they will give you as an acknowledgment of the morality of that Englishman's cheque as much German money as will enable you to treat yourselves like a Duke or diamond mine owner that night at dinner. Do not let us tamper with the value of the sovereign.

With regard to the dimensions, the weights and measures, I absolutely agree with Sir John Wolfe Barry in what he said; in fact, I would go further. I personally, in the course of my business as an engineer, have a good deal to do with calculations, and I have the greatest horror of this decimal system. I will tell you a story which will point to what I mean, rightly or wrongly. A Chancellor of the Exchequer received a deputation on the subject of the decimal system. The deputation was composed chiefly of commercial men, who, although they knew very little whether they meant the decimal system or the metrical system, honestly believed in what they had to say. They proceeded to describe the advantages of the decimal system. He said, "Gentlemen, do not waste your time; I have devoted immense thought to this subject and am a convinced believer in the decimal system." But these gentlemen felt themselves obliged to say something, and they urged that they could put before him certain points which would strengthen his advocacy of their cause. "Oh, well," said the noble lord, "by all means put your views before me, but, remember, I am a convinced believer, after great thought, in the decimal system." The Chairman then explained fully in what way *everything* would be worked out with the decimal system. "Most *interesting*," said the Chancellor of the Exchequer, "but tell me what

Mr. Tannett Walker.

are those beastly little black dots?" Now, gentlemen, that is my view. I fear those beastly little black dots. I have a greater fear that my draughtsman will put a black dot at a wrong place, than I have a fear that he will say that twice 7 makes 15. Therefore, I have the greatest confidence in the old system, which, like a man, carries its individuality upon its face.

The man who cannot look upon life as an equation is the man who always comes to grief, either financially or in some other way; and I prefer the person who makes his calculation in the form of the old-fashioned equation, which naturally results in a vulgar fraction. If a man has a mind which puts its calculations in the form of a vulgar fraction, he has something to look back upon and to check his thoughts by when he is making his calculations; but the man who begins with the figure 1 and a thousand noughts, and wonders whether he is to put the dot here or there, is more likely to get wrong than the man who puts his $\frac{15}{16}$ths in one place and his other figure elsewhere; it is a thousand to one, if he is only patient enough, that he will eliminate the $\frac{15}{16}$ths when he looks for it. When he has made his calculation he has a definite thing in front of him, and the vulgar fraction reminds him of what he was thinking about when he made his calculation.

I believe it would be very foolish for this country, with its continually decreasing trade with foreign countries—not decreasing because we are decreasing in our ability, but decreasing because foreign countries are increasing their powers of manufacture—I believe it would be very foolish, as Sir John Wolfe Barry said, for this country to copy these people merely for the sake of being in with every one. England has never been a nation that has worked on the line of always trying to square the public; we generally try to go on our own lines; and although we may be very old-fashioned, and very muddling, and blunder-headed, we generally come out at a reasonably good place when we have finished. I do hope that we shall pause, and that we shall all get to understand exactly what we do want—whether the unit to be used is to be the metre—that metre which was supposed to be a measure of the surface of the earth, but which has since been found to be incorrect. I do hope we shall pause before we build our future hopes of success on any imaginary dimensions that may have been settled by other countries. As long as we can persuade foreigners to buy our machinery made to those miserable inches and those miserable feet and those miserable twelfths and fifteenths and sixteenths, I hope we shall not alter our dimensions merely for the sake of being able to measure the shaft of our motor-car, which comes from France, in French dimensions.

Mr. Shoolbred.

Mr. J. N. Shoolbred: Sir John Wolfe Barry in his remarks went to the root of the question when he stated: that what we have to consider lies in the difference between the duodecimal and the decimal systems. I cannot think that the duodecimal system, divided in the way in which Sir John gave us an illustration, is in any way simpler than the decimal system. My experience in France and in other Continental countries, as well as at home, during many years, leads me to think that the metric system is much the simpler of the two.

Mr. Shoolbred.

Remarks have been made by Sir Frederick Bramwell, and others, as to the inconvenience which would be entailed, particularly in mechanical workshops, by the introduction of the additional measuring units of the metrical system. Sir Frederick illustrated this by reference to a paper by Mr. Coleman Sellers, presented, in 1880, to the American Society of Mechanical Engineers. Pointing out the extreme inconvenience, as well as cost, of the introduction of the metric system into machine shops in that country, that paper, Sir Frederick said, resulted in a recommendation to Congress not to introduce the metrical system. Sir Frederick, however, did not allude to the great change of opinion, which has taken place in the United States during the last twenty years, as is evidenced by the fact that Congress has at present before it a measure for the compulsory introduction of the metric system. Its introduction is to be gradual. In the first instance, the metric system there is to be imposed upon the Government Departments themselves, and in the second place, after a longer period, upon the general public; but no penalties whatever are attached to non-compliance therewith. Some arrangement of that kind would, in this country, relieve the compulsory introduction of this system of the drastic character which has been referred to. Furthermore, I am informed, apparently on good authority, that Mr. Sellers himself has not altogether abandoned the use of the metrical system in his own shops, as Sir Frederick Bramwell mentioned: perhaps Sir Frederick would ascertain whether this is so, or not.

With regard to the difficulties which have been mentioned by Sir Frederick Bramwell, Sir John Wolfe Barry, and others, as to the introduction of the metrical system, it appears to me that they are answered most effectively by the presence of such a large audience in this room. For the great, nay almost abnormal, increase of this Institution during the last twenty years is largely due to the fact that the C.G.S. units, which were first introduced by the British Association (and which mean practically the metric system), have since become the universal mode of electrical, and physical measurements, throughout all countries. So much so, that any person, in any country, may take up and understand the electrical and physical measures, in any text book, irrespective of the language in which it may be written. We ourselves, here, are therefore practically an example of what can be done, and that voluntarily, and without any compulsion, towards the introduction of the metric system.

Colonel Crompton.

Colonel R. E. B. Crompton, C.B.: A good deal of irrelevant matter has been brought into this discussion. Surely the first question for us to consider is whether a change to the metric system is possible, even if it is desirable. Although we all desire uniformity and simplification in our calculation of money, weights, and measures, we must remember that changing any linear standard affects the mechanical engineers of the world far more than any one else, and of these engineers America, England, and her Colonies constitute the majority. How then will the arguments that have been here used in favour of the metric system be received in America? Although Mr. Shoolbred has said that in America there are signs of a change of opinion in favour of the metric system, I

Colonel Crompton.

must emphatically contradict him. The state of American opinion is well shown from the recent discussions on this subject of papers read before the American Society of Mechanical Engineers which were well reported in *Engineering* of January 23, 1903, pages 104 to 107. The discussion on Mr. Halsey's paper on the metric system lasted two days and was followed by numerous letters from manufacturers, all showing that no one in America favoured the metric system. A very strong letter wholly condemning the metric system of linear measurement was read from Mr. Charles T. Porter, one of the most respected past-presidents of that Society. This letter deserves to be reprinted in full in this discussion.[1]

[1] The following is the letter referred to, as quoted by *Engineering* (1903, Vol. 75, No. 1934, pp. 106, 107) from *Engineering News* (December 25, 1902) :—

"SIR,—'ABSURD!' Yes, that is the word with which the Committee of the American Society of Mechanical Engineers on the metric system fitly characterised and contemptuously dismissed the Bill, now before Congress, making our system of linear measurement illegal. That word was the necessary conclusion from the facts presented in the report of the Committee.

"The promoters of this measure were very properly excused on the ground of ignorance. If they had the least idea of what they were doing, of the unapproachable excellence of the system of linear measurement on which they were laying their hands—an excellence which is briefly outlined in the report of the Committee, but which can be realised only by those who are familiar with its use—their advocacy of this Bill would be without excuse, or rather it would be an act of which they would be incapable.

"To begin with, I arraign the metric system itself as absurd. The idea on which this system was founded was big and childish; one which no people except the French could ever have thought of. To them it seemed sublime. They would take for a unit $\frac{1}{10,000,000}$ quadrant of the meridian, or the distance on the earth's surface from the Equator to the Pole, and make this unit of a grand decimal system of measurement of everything on the earth and in the heavens; and from this they would derive a unit for another grand universal decimal system of weight. After the metre had been materialised in a metal bar, and this bar had been legally proclaimed to the world as the said universal unit, it was found to be too short, and the absurdity of this visionary fantasy stood exposed. The metre is merely an arbitrary unit, as any unit of measure or weight must necessarily be.

"This performance would be too ridiculous to notice were it not for these two facts. The metric system is still proclaimed to be the grand universal scientific system of weight and measure, and many merely theoretical minds, and I am sorry to say some practical mechanical minds also, in this country are dazzled by its brilliant pretensions. The fantastic foundation is also a key to the character of the system. We shall see that as a whole it is the product of the same merely theoretical and visionary minds.

"Secondly, the metric system is absurd in confounding together weights and measures, things which are entirely dissimilar and unrelated, and applying the same system of division to both. Universality was the hobby and the blunder of its schemers. Thus we have this result. Physicists deal with minute quantities, and do not measure, but only weigh. In the free exercise of their right to choice, they found the gram and its decimal divisions to be admirably adapted to their use; their work lying within the natural field of the decimal system. From this they jumped to the universal conclusion, which is not merely unscientific, but is senseless, that the metric system must be equally suited to everything: to things, large as well as small, and to measurements as well as to weight. *But English-speaking people who measure do not agree with them. Therefore* these people must be deprived of their right

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In Mr. Halsey's paper the following words occur:—

"The chief value of a standard lies in the fact that it is adopted, that it has become a part of our daily lives, and works so smoothly that we are scarcely aware of its existence. For example, the value of pipe thread standards is not represented by the tap and dies in the hands of pipe makers and fitters, but by the fact that because the threads are standardised, pipe fittings can be made by the million at trifling cost. The cost of changing our pipe thread standard is not represented by the cost of new taps and dies, but by the confusion involved in getting from one standard to another, a confusion which will last until all

of choice, and compelled by law to take the medicine that these doctors think will be good for them. This illustrates a radical absurdity of the metric system, applying one universal method to everything.

"Confining our attention now to measurement, with which mechanical engineers are chiefly concerned, I note, thirdly, that the metric system is absurd in ordaining a single unit of measurement for everything, from the least to the greatest, when all other systems employ a number of units, each one especially adapted to a larger or a smaller field. This absurdity stands confessed. The metricians found themselves after all compelled to employ three units, the additional ones being the kilometre for land measures, and the millimetre for mechanical measures, thus making necessary the use of three decimal points.

"Fourthly, the metric system is absurd in forbidding the use of division by continual bisection, the natural method which first occurs to everybody, and which possesses important advantages, as mentioned in the report of the Committee; thus interfering with individual freedom of choice, which is a natural right, and ordaining for universal use the decimal system of division only, the proper field of which is in the expression of very small or fractional quantities, and which is wholly unsuited to express large dimensions.

"This absurdity is realised in its utmost aggravated form in mechanical measurements, in which every dimension, however large, it was found necessary in the metric system to express in millimetres, the smallest unit, ·03937079 inch. Thus, 38 feet are 11,558 mm., and these five figures and two letters must be written. Nice to remember! We might just as well be compelled to express all divisions of the circle or of time in seconds.

"But, say the metricians, we want uniformity. Well, in the English system of linear measurement we have uniformity. It presents the very ideal of uniformity. Throughout the United States and the British Empire, all English-speaking people on the globe, in their great variety of occupations, every man who measures any thing for any purpose, all employ the same identical system of measurement. Its great practical excellence has compelled its universal adoption by men free to use the metric or any other system if they want to, and with the same freedom of choice this excellence will make its use universal.

"The proposed law excepts land measurement. The same reason would cause all measurement to be excepted from it. Yea, they are tenfold stronger in the case of mechanical measurement. It would produce quite as great confusion or chaos in mechanical as in land measurements—indeed far greater. Its disastrous effects in cutting us off from our mechanical past, and in annihilating our standards and our literature, would be inconceivable, and all for what? Echo answers, What?

"A judicious law, giving to this nation the same uniformity of weights that we now enjoy of measures of length, would doubtless be hailed as a benefit. Adherence to the proposed law, applying the metric system, which confounds measures and weights, and applies one arbitrary system to both, will bring our legislators, sooner or later, to realise that our system of linear measurement is interwoven with the life of this people; that they realise its inestimable value, and that they are fully able to maintain it.

"(Signed) CHAS. T. PORTER.

"Montclair, N.J. December 16, 1902."

existing steam, water, and gas pipes have disappeared, and it will not be lessened by putting off the change until it is brought about at the suggestion and convenience of manufacturers. It is because of our standards and our standardised methods that American mechanical industries are great. It is in this that we lead and by this sign we conquer. It is this that distinguishes us from the remainder of the world, and having the lead which such things give us, we are asked to abandon it and line up in the race afresh, and this in the name of progress." Colonel Crompton.

He further went on to say that no mechanical engineering society had said a word in favour of the metric system.

If this is the state of things in America, and I believe that it is so, we in this room must see the enormous importance and immense sum of money involved in a compulsory change of linear measure when applied to the two great mechanical nations of the world.

For the reasons above given, the possibility of simplifying money, weights, and measures of capacity by decimalising them is of quite a different order of possibility to that of changing linear standards. Sellars point out that the capital expended in measuring plant, which is not likely to be scrapped or made obsolete in any way except by this change is, in the English-speaking and inch-using countries, America, England and her Colonies, many times greater than the capital already expended in the countries using the metric system. Of course this applies equally to Professor Stoney's proposal to alter the inch. If any change is possible in the future, it will be that of altering the millimetre to become the exact 25th of the inch. The tail cannot wag the dog, and in this particular instance the tail is the metric system and the dog is the inch.

Another point where the inch divided on the binary scale is superior to any system based wholly on decimal division is that of screw threads. It is found in practice awkward and inconvenient to use decimally divided leading screws for lathes, so that even in countries using metric systems most of the screws used have threads of Whitworth pitches based on the binary scale, and whenever metric leading screws are used these also are to some extent on the binary scale, that is to say they use 4-millimetre, 6-millimetre, and 8-millimetre pitch and so on; but even then they are more inconvenient than Whitworth leading screws based on the inch divided on the binary scale. Of course this defect has nothing to do with the metric system, but shows the inferiority for this purpose of the decimal to the duodecimal system of division. The result of this is that in countries using the metre we find two sets of measurements on one drawing, the metric for most of the details, but Whitworth pitches and dimensions specified for the screws. The Whitworth scale of pitches at so many threads to the inch is simple and easily remembered, but on the metric system, in order to avoid confusion and mistakes, the names of the pitches must be given in tenths of millimetres, so that we find the change wheel tables for their lathes naming the pitches in tenths of millimetres such as $\frac{77}{10}$ths and so on.

Speakers who have dwelt so much on the increased number of

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figures and waste of time involved by the calculations when using our present system of weights and measures quite forget that since the use of the slide rule became so universal the time taken in converting from one system to the other is trifling. No electrical or mechanical engineers concern themselves with the obsolete measures concerning which so much valuable time has been wasted by some of the speakers. Practically we only have to deal with the inch divided on the binary and decimal scales, the foot, the yard, the pound, the ton, the gallon, the cubic foot and cubic yard. There is no difficulty in making calculations or estimates in these measures, and whenever it is required in converting them into metric measures we can do so by a single operation of the slide rule or calculator. The time taken in doing this is inappreciable, in fact less than the usual time taken in considering the position of the decimal point, and in practice I do not think that English and American electrical engineers users of slide rules do find any appreciable waste of time in bringing together electrical calculations worked out on the metric system with the mechanical details of their machinery based on the inch.

-Gen. ber.

Major-General C. E. WEBBER: There is one point which Sir Andrew Noble mentioned that, I think, cannot be passed over, namely, that the real question underlying and behind this one as regards the brains of the nation, and as regards the education of the rising generation, is whether we are going to teach them to think in tenths or twelfths; that means, are we going to divide the pound sterling into thousandths? I remember sitting next to Mr. Gladstone many years ago, and asking him how he would agree to 10d. to the shilling. His answer was, "What would become of the apple-woman's penny?" What Sir Frederick Bramwell said at the last meeting reminded me that in the present generation the question of tenths and twelfths is largely sentimental.

I should like to remark that there is a great body of professional men in this country, numbering upwards of 33,000—I mean those connected with the building trades, architects, surveyors, and members of building firms—who are using inches, feet, and yards every day of their lives, who have to be consulted in this matter quite as much as the engineering world, which contains not half the number of professional men who use those measurements to the same extent. The only thing for which I am sorry this evening is that we have no representative of the building trades here who has worked both in France and England as I have. I should like to hear from the side of the man who has never worked with decimally divided measurements and money (as one does in France) whether he really appreciates the number of figures that are entailed by calculations in the ordinary working out of quantities and costs by the two systems. If any one will take the trouble to write down for themselves a few simple problems of cubic contents in earth-work, in brick-work, in wood-work at varying prices, and calculate them out in money, he will find that the number of figures in our duodecimal, as compared with the decimal system, is 61 to 23; that is, the actual number of figures he would have to put down on paper are nearly three times as many in the former as in the latter,

and when you come to remember the enormous number of calculations that have to be made, you will realise the convenience and saving of labour in connection with quantities and prices in building construction carried out by professional men in France as compared with this country. Maj.-Gen. Webber.

Mr. A. E. LEVIN: It is sometimes said that the comparison is between a duodecimal and a decimal system. Surely to call our English weights and measures a duodecimal system is gross flattery. We have twelve inches in a foot, it is true, but in the Avoirdupois weight there is not a single multiple of three, so that if we wish to divide a ton into three equal parts we must go to the fraction of a grain. If any division by three is necessary, which brings us below an inch, our binary subdivision of the inch becomes absurdly inconvenient. Even to one-sixty-fourth, no exact third of an inch can be expressed. Another objection which is sometimes made to the metric system is that the multiples and sub-multiples are expressed by Greek and Latin words. I have never yet heard that the Greek and Latin origins for their names have spoiled the popularity, say of the telegraph or the omnibus. Colonel Crompton has spoken of the enormous cost of the standards, the templates and gauges represented by our inches and feet in mechanical workshops. But surely the capitalised value of the mental labour which is spent and wasted in calculations in feet and inches, in acres, in miles, in chains, in the five and a half yards which go to make a rod, is enormously greater than that capital which is locked up in the foot and the inch. Mr. Levin.

Prof. R. H. SMITH (*communicated*): I can testify to the fact that errors in reading from drawings such dimensions as $12\frac{1}{2}''$ instead of $1'\ 2\frac{1}{2}''$ are more frequent than those arising from misplacing the decimal point. This latter is an error of no great practical importance because it is one of such gigantic amount, that it cannot be carried on into practical operation. The error of reading $12\frac{1}{2}''$ in place of $1'\ 2\frac{1}{2}''$ is entirely due to our division of the foot not corresponding with our system of numeration. If this were duodecimal, or if the base for division and multiplication of unit measures were the same as that for numeration, whatever that base might be, such mistakes could not occur. Prof. Smith

British measures are not duodecimal. They are divided up by 2, 3, 5 and 7 with extreme irregularity. The want of system appears not only as between measures of one kind and those of other kinds: it is equally extravagant in each set of one kind. The mile is divided by 8, then by 10, then by 2×11, then by 3, then by $2 \times 2 \times 3$, then by $2 \times 2 \times 2 \times 2$. The ton is divided by 2, then by 10, then by 4×4, then by 7.

If we had a real SYSTEM of measures whose base was $12 = 2 \times 2 \times 3$, and if our numeration were also founded on this base, this would be, no doubt, better than a decimal system: but one whose base was $2 \times 3 \times 5 = 30$ would be still better, and with very little practice all educated persons could easily work their arithmetic by powers of 30. In the base 12 there is *no* essential advantage gained by the repetition *of the factor 2*. There is no ground reason for halting between the

of. Smith. bases $2 \times 3 = 6$ and $6 \times 5 = 30$. If 30 be really too big for our average arithmetic intellect, and 6 be too small to utilise it fully, we have to choose between $2 \times 2 \times 3 = 12$ and $2 \times 5 = 10$; that is, to choose between omitting the factor 3 or 5.

Exactitude, as distinguished from accuracy in the sense of freedom from mistake, is of no real value in any kind of practical work. The data for calculation are never known with exactitude, and a greater degree of minute exactitude in calculation than exists in the data is worse than delusive, it is often materially injurious. For this reason the selection of a base because of its richness in exact fractions of simple form is, in my opinion, of no real practical importance. The division of the circle in 12 and 24 parts is natural and easy; but this seems the only good ground for regarding 12 as a superior base. But all practical calculators know that measurement of angles by 360° to the circle is an unmitigated nuisance. One set of calculations demands the "circular" or π measurement; while in another the direct measurement of angles by their tangents or sines is infinitely more convenient. Measurement by 360° is NEVER useful: it ALWAYS involves labour lost.

What is of absolutely essential practical importance is that the base of written numeration should be the same as that on which measures of all kinds are systematised. For written numeration all nations throughout the world have adopted the base 10, and, for better or worse, are certain to adhere to it immovably. As this cannot be changed, the unavoidable conclusion is that measures should be decimalised.

It seems quite irrelevant to discuss whether the yard, metre, foot, inch or millimetre is the better standard unit. For the manifold purposes of industry it is absolutely essential to use various sizes of units of each kind. The output of a mine cannot be measured in lbs., nor bread be sold retail by the ton. For wire drawing and gauge fitting the inch unit is at least a thousand times too big, while for land surveying it is at least a thousand times too small. Thus no one unit of each kind can be said to be even approximately the best, or, indeed, to have intrinsically any advantage over another. What practical working convenience urgently demands is that all the various sizes of units found convenient in practice for each kind of thing should be interrelated by similar numerical ratios as are adopted for those of other kinds of things, and for convenience in decimal calculation all these ratios should be decimal.

The units of the metric system have no intrinsic superiority over others. The intrinsic superiority of this system lies in (1) that it is strictly systematised on one base ratio throughout, and (2) that this base ratio is 10. No other system exists which has either of these two advantages, and these two are all that are wanted or can be rationally conceived of. Any number of systems fulfilling these two essentials with other units may be devised; and, if universally adopted, any such system would be equally useful and convenient. But the metric system is already used by a large proportion of the industrial and scientific parts of the human race, and no possible advantage can accrue from its wanton destruction. For no other can possibly be better in practical essentials except in substituting for 10 the base 12

or 30 for measures and written numeration alike, and this latter is humanly impossible. Prof. Smith.

Lieut.-Col. Crompton's argument that the existing capitalised interest embodied in inch plant is greater than that in metric plant is the only strong argument against the universal adoption of metric measures. The following considerations, however, tell against it. A very large amount of mental and other labour has been embodied in the calculation and printing of logarithmic and other decimal tables, and if the necessity for the same base in numeration and in measures be recognised, and if 10 be abandoned, all this labour would be thrown away. Again there is a much larger proportion of the metric than of the inch plant that is wholly modern, and up to modern requirements of efficiency. A much larger proportion of the inch plant is near the end of its life, partly because of being worn out, and partly because of antiquation of pattern. Plant of all kinds, and especially that of antiquated type, is being scrapped rapidly, while the question of changing over to metric measures is one essentially for the future. It cannot be judged fairly, simply in view of the inconveniences and losses to be borne in consequence of it during a period of 5 or 10 or 20 years. It is a change the advantages of which will operate throughout centuries; it is quite improbable that any new alteration would be demanded for 1,000 years. Besides a comparison between the two parts into which existing plant may be divided, that whose life is already well spent and that whose life is just beginning, there is also to be considered the new plant, not superseding old scrapped plant, that will be laid down during the next 100 years, not only in Europe and North America, but also in China, Japan, Siberia, India, Australia, South Africa, and South America. The advantages to be gained accrue in respect of all this new plant recently laid down and to be created during the next few centuries, while the losses incurred by the change affect only the plant, much of which is almost dead already, and of which not a single ton will be in existence 20 or 30 years hence, except stored in the corners of historical museums.

Mr. Albert Campbell (*communicated*): Although I would advocate in the strongest possible manner the immediate adoption of the metric system, I think there is one defect in it which should be remedied before the British public is asked to accept the system. I allude to the cumbrous naming of most of the metric weights and measures. If we are to throw away handy words like *inch*, *pound*, *ounce*, and *mile*, we must replace them by something shorter, clearer, and better than the French centimetre, kilogramme, decigramme, and kilometre (or Anglicised centimeter, kilogram, etc.). An inventor with linguistic imagination is wanted here. Mr. Campbell.

It is to be hoped that when the metric system comes in, the *coinage* will also be made decimal. This would be a much easier matter than most people imagine, for it would be only necessary to make the *half-sovereign* the unit, and alter the copper coinage to make 10 new pennies in a shilling. The silver and gold coins would then only want renaming, and prices of 1d. a yard, or 1d. a pound would be very nearly equivalent to a new penny for a metre or a half kilogramme.

npbell. This would be a distinct convenience to the less intelligent buyers, whilst to keep £1 sterling as unit would involve a much more difficult change.

nens. Mr. ALEXANDER SIEMENS, in reply, said: Gentlemen, I will begin by referring to Colonel Crompton's remarks. I was prepared to hear him say something about screws, so I had some screws made for his special delectation, and while I am replying to the other speakers I hope Colonel Crompton will look at them. There are four screws. Two are made on a lathe with a metrical lead, a 4 mm. leading screw, and two are made on a bench with eighth of an inch leading screw. Two of the screws are of ⅛ in. pitch, and two of the screws are of 4 mm. pitch. Then two nuts have been made. The nut for the 4 mm. pitch is made with a French tap, and the nut for the two screws of ⅛ in. pitch is made with an English tap. I should like Colonel Crompton to tell me which of the two screws are made on the mm. screw and which on the other. The screws are numbered, and I have a paper here on which it is stated where the various screws were made. This is a practical answer to the objection that English cut lead screws have to be scrapped, if metrical measures are introduced.

Colonel Crompton has stated that much extraneous matter has been brought into the discussion, and I fully agree with him. The title of the paper was, "Discussion on the Metrical System," but Sir Frederick Bramwell began by bringing "compulsion" in, a subject about which I expressed no opinion. I did not want to make any comparisons; I simply wanted to discuss what could be said for the metrical system, and what could be said against it. The question of compulsion was brought in, because I quoted what a Select Committee of the House of Commons had said. It was not my idea: it has not been the idea of the advocates of the metrical system, but it was the deliberate opinion of the Select Committee of the House of Commons—or rather two of them—that it would be to the advantage of this country to introduce the metrical system.

The reasons why the metrical system is advocated, if you omit all those external things, are based on its convenience for international trade, and for everyday use in calculation. International trade I mentioned in my remarks, although Sir Frederick Bramwell thought I did not. I said that intercommunication between countries has so very much increased during the last century, and especially during the last part of the century, that it is very desirable that a general international system of weights and measures should be adopted. That is an opinion which has been come to by other people also, notably by the Select Committee of the House of Commons in 1862. They said that it would not be desirable to create a national system, as, sooner or later, Great Britain would have to join an international system. The same conclusion was arrived at by the German Committee which was appointed about the same time. It was an instruction to this German Committee that they should find a national system, not an international one: but after they had investigated the subject a short time they came to the conclusion that it was *absolutely* necessary, if any change was to be made, that it should be *for an international* system, and that there were only two such systems

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from which to choose, the English or the French metric system. They went into the subject very thoroughly, and the result of their deliberations was that they adopted the metric system. If this Commission had any predilection for either system, it was in favour of the English, because you must recollect that in 1862, forty years ago, everything English was considered in Germany as something extraordinarily good, and you could not give a greater recommendation than that the article was English, so that the prejudice was all in favour of England and against France. But this Commission appointed from all parts of Germany—it was not united at that time—agreed that it would be better to adopt the metrical system. After all there is some weight in that.

Not only Sir Frederick Bramwell, but other speakers have mentioned the Greek and Latin names. Mr. Levin really answered that question. The fact of the matter is that you do not use so many names. In length you use the kilometer, metre, and millimetre, and sometimes the centimetre. You use the kilogramme and gramme, and you use the litre, and for everything else you use multiples or squares and cubes of these units. We should look at the experience of other countries who have introduced the metric system—for instance, Sweden, Holland, and Germany—and have tried to invent special names in order to avoid these Greek and Latin names. In Germany they made an additional alteration. They did not adopt the kilogramme as a standard, but the pound: that was because the pound, the metrical pound, the 500 grammes pound, had been introduced long before. In 1840 it was adopted for the Customs Union, and in 1860 it was made compulsory, so they kept it in 1870 as the unit of weight instead of the kilogramme. Connected with the introduction of the pound there is a very interesting point. Some of these little German countries thought they would improve on the French system: therefore they did not divide the pound into 500 grammes, but into 30 ozs., some into 32 ozs., and others into 10 ozs. They thought by that means they would introduce a great improvement. But the general experience showed that it was much better to adhere to the pure decimal system. In Germany the metrical system of weights and measures was made compulsory in 1872, but already in 1877 the pound was given up as the unit of weight and the special names were dropped. In Holland and Sweden likewise the special names have been given up, and the pure metrical system is in use in all these countries.

I am afraid I cannot answer all the various speakers in detail, but the great point which has been brought forward against the metrical system is the decimal division. I think in this respect the objectors have put the cart before the horse, because the people who devised the metrical system did not force on an unwilling world the decimal arithmetic; but they divided the metre into decimal parts, and connected the various units on a decimal basis, because decimal arithmetic has been in use and will remain in use. I should like to ask Sir John Wolfe Barry, who is such an expert in the English and so-called duodecimal system, whether he has ever had any serious practice in the metrical system, because he will find that the moment you begin to really work in that system you will find as many short cuts in the

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metrical system as there are in the English. It depends entirely upon what you are used to. Personally I have been entirely educated in feet and inches. I left Germany before the metrical system became compulsory, so that I must not be held up as a bad example on the other side: but in my opinion it is certainly more convenient when you have everything connected decimally the same as in ordinary arithmetic. Sir Frederick Bramwell on the last occasion was quite wedded to compulsion. He said the metrical system compelled you to use decimal fractions. The metrical people, if they can avoid it, never use a fraction at all, and that is just one of the advantages of the system. If you have to do with big weights you talk about tons or kilogrammes: if you have to do with small weights you use grammes: and if you have to use chemical weights you use milligrammes; but you never use fractions at all if you can avoid them: that is one of the advantages. On the other hand, why should you not use vulgar fractions? Sir Frederick Bramwell really gave himself away; he said, "I am not against decimals at all: I use decimal fractions wherever they come in handy." That is the answer. If decimal fractions are convenient, I use them; if vulgar fractions are convenient, I use vulgar fractions; there is nothing in the metrical system against the vulgar fraction. Why should not you talk about half a kilogramme? Reverting again to Colonel Crompton's speech, he ridiculed the German screw gauge which had $\frac{77}{100}$ths of a millimetre. I am sorry I have not the British Association screw threads here which Colonel Crompton recommended. There every one of the sizes has two decimals of a millimetre.

Colonel Crompton.

COLONEL CROMPTON: That is why I am condemning the millimetre.

Mr. Siemens.

MR. ALEXANDER SIEMENS: But in the inch scale you have given four decimals. I certainly do not believe that the period of transition would be so difficult or cause so much commotion as some people think. But one of the difficult points, no doubt, is the screw thread. All this talk with regard to Germany and other metrical countries using the Whitworth thread sounds convincing, but as a matter of fact there are no end of threads in Germany, and there are no end of threads here in this country. What has been the conclusion arrived at by the several Committees on screws, not only the Committee of which Sir Frederick Bramwell and Mr. Crompton were members, the British Association Committee, but the War Office Committee, the International Committee, the German Engineers Committee, and other committees in various parts of the world: what has been the result of their deliberations? The result of their deliberations was, that it was practically impossible to make screws fit which had been manufactured by two different manufacturers unless standard screws and standard cutters were deposited in some place where everybody could go and compare his screws with them. At the present time the War Office is putting up a standard screw machine at Bushy Park in the National Physical Laboratory, where all screws which are to be eventually supplied to the War Office are to be standardised, where people can obtain leading screws so that they can make War Office screws. Therefore the exact size of the pitch of the thread, whether in metrical measure or not, does

not matter, because eventually it comes back to the gauges and templates and standard taps. The screw difficulty, although it looks very formidable, is therefore really nothing much. Mr. Siemens.

I have one more thing to say about the subdivisions. It is always best to try and ascertain the opinion of outside people. You will find that the Commissioners for the standards of weight and measure appointed in 1841 strongly recommend the decimal division of the pound. I will read you a letter presently which has reference to that. Then there is an Institution called the Liverpool Cotton Association. They have been selling cotton, I suppose, ever since it was imported, and used, by the point. A point used to be $\frac{1}{64}$ of a 1d. Every now and then they quoted even by half a point, but after using this binary subdivision for all these years they have come to the conclusion that this binary subdivision, which has been praised so much, is not sufficiently convenient, and from the 1st of October, 1902, they have begun to make their quotations in $\frac{1}{100}$ of a 1d. They have gone over to the decimal from the binary, because they did not find it convenient.

In conclusion I will read you a letter which I received about weights and measures in a little village in Cumberland :—

"CUMBERLAND, *January 20th*, 1903.

"NOTES ON METRIC SYSTEM.

"(1) We are told that for years after the adoption of the metric system everybody would be continually doing mental conversions. But they are continually doing them *now*. Lately I told a man the depth of a shaft in feet, and he had to turn it laboriously into fathoms before he could understand it.

"(2) Another man put a cistern in his upper storey, and, wanting to know if his beams would bear the weight, asked me the weight of a cubic inch of water. He expected an hour's calculation, but by turning his inch measurements roughly into metres, or rather decimetres, I solved it in twenty seconds in my head.

"(3) I believe Sir F. Bramwell maintains that columns of tons, cwts., qrs., and lbs. are easier to add up than tonnes and kilograms. I am clerk to a mining company whose head office is in Liege, and have abundant experience of both kinds of sum. I think one page of cwt., qrs., lbs., about equivalent to three of kilos. When it comes to calculating the amount of zinc in them from a given percentage, the British system becomes practically impossible.

"(4) We are told that the pound avoirdupois is essential to British comfort. Here in the highest village in England the pound is little used. Apples and many other things are priced at (for example) '8d. a quarter,' meaning quarter-stone. The stone here was 16 pounds a century ago, now it is 14; but I have often seen small shopkeepers use a four-pound weight for a 'quarter-stone.' Evidently people who will sell you four pounds for a quarter the price of fourteen are not likely to be much troubled by the difference between two pounds and a kilogram. Evidently also they find it convenient to use a larger unit than a pound, and would therefore probably like the kilogram.

"(5) Tea, coffee, etc., are priced by the pound, but are always put up in quarter-pound packets. This shows that the hectogram would be a convenient unit.

"(6) In our laboratory is a set of weights marked in 'septems.'

Mr. Siemens.

When I came here no one knew what they were, but I found a 'septem' was 7 grains, $\frac{1}{1000}$ of an avoirdupois pound. This shows that attempts to decimalise the British measures fail.

"(7) Lead ore here is measured by the 'bing' of 8 cwt. Zinc blende, obtained at the same time from the same mines, is weighed in tons. The rough 'bouse' from which both are extracted is measured in 'shifts'; a shift is supposed to be 4 tons. Miners are paid by the 'cubic fathom' of 6 × 6 × 4 feet. The present mining company, despairing of ever putting this chaos straight, is gradually replacing all these measures by kilograms and cubic metres.

"(8) A gill in the schoolbooks is ¼ pint; in trade here it is ½ pint.

"(9) When we buy timber it is invoiced thus: '1 st. 1 qr. 16 deals 4/6 at £16 a standard.' A 'standard' is 120 'standard deals,' each of which is 72 × 11 × 3 inches in England and 144 × 9 × 3 in Ireland. The 'standard' is divided into 4 quarters, each of 30 'standard deals.' This bewildering system (which is not to be found in *Whitaker's Almanack* nor any other book of reference within my reach) is confined to the wholesale trade; smaller dealers sell by comparatively intelligible units of 100 square feet.

"(10) The mugs distributed to the children at the Coronation were just ½ litre.

"(11) A Swedish lady here ordered a dress from the village dressmaker and gave the measurements in centimetres. The dressmaker begged a metre-tape from me and made the dress without the least difficulty."

I think that shows how difficult it is to learn the metrical system.

The President.

The PRESIDENT: I will now ask the meeting to pass a cordial vote of thanks to Mr. Siemens and Sir F. Bramwell for their contribution to the discussion.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected, viz.:—

Members.

Clement Johnson Barley.
Prof. W. C. Unwin, F.R.S.

Associate Members.

Robert Alexander Raveau Bolton.
Claude Greener Cadman.
Geo. Henry Clapham.
Alfred Lawrence Eugene Drummond.
Frank Edmondson.
Geo. Emerton Higginbotham.
Charles James Jewell.
Reginald Keble Morcom.
Ernest Probert.
Charles Edward Squire.
Walter Noble Twelvetrees.

Associates.

George Edward Anness.
Alfred Anthony Blythen.
James C. Cunningham.
Lewis William Dixon.
Samuel Slack Foster.
John Godfrey Y. D. Morgan.
Edward Phillips.
James Arthur Sykes.
Robert Alexander Ure.
Maximilian J. L. Weston.

Students.

Frederick Creedy.
Erich Egon Edmund Dormann.
Frank Donald Howard.
Harold Carnegie Jenkins.
Arthur Henry Knight.
Archibald Charles Lock.
Herbert Richard Marr.
Douglas William Munton.
Chas. Wm. George Nelson.
John A. G. Ogilvie.
Frederick Handley Page.
Enrico Arthur Pinto.
Frank Bennett Preston.
Carl Hubert Sanders.
John Henry Charles Searle.
Claude Theodore Sielis.
Frederick Swarbrick.
Richard Henry Turrall.
Leslie Wainwright.
Herbert Wilson.
Wm. Francis Wolfe.
Ernest Benjamin Woollan.

The Three Hundred and Eighty-fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 8th, 1903—Mr. JAMES SWINBURNE, President, in the Chair.

The minutes of the Ordinary General Meeting held on December 18th, 1902, were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that these names should be suspended in the Library.

The following transfers were announced as having been approved by the Council —

From the class of Associate Members to that of Members—

Arthur Thomas Cooper.	Joseph Robert Woodruffe Gardam.
Forrester Ferguson Ferguson.	Norman Rheam.

Walter Adolph Vignoles.

From the class of Associates to that of Members—

Major Walter Arthur John O'Meara, R.E.
Lionel Hugh Kenmure Stotherd.

From the class of Associates to that of Associate Members—

E. E. Benham.	Chas. Keeble.
Jas. Brown.	T. Kerr-Jones.
John Brown.	Lionel Jas. Langridge.
Thos. Carter.	Robert Andrew Miles.
Alfred Charles Cossor.	Edwin Morgan.
Arthur W. Cox.	Hugh Bernard Player.
Ernest Holmes Llewelyn Dickson.	Oliver Archer Richardson.
Fourd Ely.	C. W. Schaefer.
Cecil Chas. Fowler.	Francis Sydney Shaw.
Reginald Wilson Gauntlett.	Eustace Graham Sheppard.
John Owen Girdlestone.	Sidney Arthur Simon.
Edmund Goolding.	Chas. Wm. Spiers.
John Gray	Leonard Geo. Tate.
Henry Human	Wm. John Thorrowgood.
Edward Henry Johnson.	Max. Jas. Eccles Tilney.

John Frederick Wakelin.

From the class of Students to that of Associates—

Arthur Buckney.	Henry F. Jay.
Jas. John Chapman.	Frank Clement Knowles.
Harold Frodsham.	Edmund Lewis Robinson.
Geo. Hicks.	R. E. S. Turnbull.

Messrs. F. C. Hounsfield and L. L. Robinson were appointed scrutineers of the ballot for the election of new members.

Donations to the *Library* were announced as having been received since the last meeting from Messrs. Macmillan & Co. and Prof. S. P. Thompson; to the *Building Fund* from Messrs. E. Coates, P. F. Crinks, H. W. W. Dix, W. Duddell, J. H. Edwards, W. Golledge, T. F. Griggs, C. W. Hacking, R. Hammond, Prof. A. Hay, A. P. Hutchinson, Captain Jackson, H. W. Miller, H. B. Mitchell, G. Offor, A. P. Patey, W. M. Rolph, J. H. Rosenthal, A. Rutherford, W. H. Shephard, J. M. Smyth, M. Solomon, A. Stroh, A. A. C. Swinton, L. C. B. Trimnell, A. S. Wilson, H. W. Young; and to the *Benevolent Fund* from Mrs. Ayrton, Messrs. C. P. Cobb, G. J. Gibbs, T. F. Griggs, S. H. Holden, Sir David Salomons, W. C. Smith, A. Stroh, W. C. P. Tapper, J. Woodside, and the Incorporated Municipal Electrical Association, to whom the thanks of the meeting were duly accorded.

NOTES OF RECENT ELECTRICAL DESIGN.

By W. B. ESSON, M.Inst.C.E., Member.

In the course of their visits to the Continent, members of the Institution had ample opportunity of studying the construction of electrical machinery as illustrated by the practice of Europe and America. Work representative of the best that has been done was everywhere open to the inspection of the tourists, and every facility was placed at their disposal for comparison of different designs.

In the course of this paper I shall have occasion to refer frequently to what was seen, and in this respect the Notes may to some extent supplement the excellent reports already furnished to the Council by the various German Visit Committees.

INTRODUCTION AND REVIEW.

By way of introduction I must take you back to the Electrical Exhibition held in Frankfort eleven years ago. At that superb show, in 1891, was to be seen the best that the Continent had done in construction up to that date, and it will be recollected that there was manifested considerable diversity in the type and form of machines.

Beginning with continuous current, there were in addition to the multipolar dynamos with slotted drums, the wheel-armature machines of Fritsche, the flat-ring machines of Schuckert, and several interior pole ring-wound machines by different makers, of which the largest and most notable was the one shown by Messrs. Siemens & Halske, running

at 80 revolutions per minute, to give 300 kilowatts. Of these, all have disappeared with the exception of the multipolar machines with radial magnets and slotted drum armatures. This is the only surviving type on the Continent, and it is now the prevailing type with us.

Ten years ago it looked as if in Germany the interior pole machine was to become a permanent type, since several manufacturers had taken it up, but it has gone with the others referred to. Though such machines have done and are doing their work in a very satisfactory manner, the design has faults both electrical and mechanical which are absent from the design which has replaced it. Accordingly, as a standard type, it has ceased to exist, and there are now made, only the occasional machines asked for by customers who wish their new plant to be uniform with what had been previously supplied.

In this country at the date of the Frankfort Exhibition the drum armature was rapidly supplanting the ring armature for all sizes of machines. The single magnet type of field was in general use by all makers, though some of the more important firms had been coquetting with multipolar designs. A paper of mine read before this Institution in 1890 calling attention to the advantages to be derived from multipolar fields elicited from some quarters a vigorous defence of the 2-pole. Slotted armatures had not come in, or, rather, they had come in and gone out again, because too costly at that date with hand coiling for bipolar fields. With the advent of multipolar machines came former wound coils and shaped bars, and the slotted armature once more made its appearance, this time to stay.

What has happened during the past ten years, then, as regards continuous-current generators is this: all lines of design on the Continent, in America, and in Britain have converged towards one form, which is likely to be permanent. There is practically but one type and one form of that type, differing merely in its proportions and constructional details; this form is to be seen in every new generating station—a multipolar machine with its magnets disposed radially and provided with a drum armature slotted on its exterior for the conductors.

Of alternators there were but few at Frankfort, the most worthy of note being the two machines exhibited by the

Helios Company and Messrs. Siemens & Halske respectively. The former gave 400 kilowatts at 125 revolutions per minute, and the latter 330 kilowatts at 100 revolutions, both machines being therefore of about the same size. Each had a stationary armature and a rotating field of radial magnets mounted on the engine fly-wheel. The armatures of both were of the pole type—that is to say, their iron cores had distinct interior projections corresponding to the number of the field-poles. The Helios machine was a modification of the well-known Ganz alternator described here by Professor Forbes in 1889. The Siemens & Halske armature was practically a large-sized Gramme ring with projections between the coils. Taken as a whole, the Exhibition was rather poor in alternators. There were few polyphase machines, while in the Paris Exhibition of 1900 there was little else.

At the same date there was over here considerable variety in alternators. There were the machines which had copper tape armatures without iron, some of these having stationary and some rotating fields. Then there were machines having flat coils laid on the surface of the armature core, these, again, being divided into those with fixed and those with moving fields. There were iron-cored armatures with polar projections and without, also armatures wound like Gramme rings. In some machines the fields were excited by a single coil, and in others by multiple coils, while last of all we had the inductor alternator of the late Mr. Kingdon, where the field and armature systems were fixed and the path of the magnetic induction was determined solely by the movement of blocks of laminated iron attached to a rotating central wheel.

The alternators on the Continent and in England were in 1891 on their probation, so to speak, and ten years have done much towards reducing the number of types. The copper tape armature is no more, that is in the manufacturing sense. Probably a few such machines are made for the sake of uniformity in the extensions to existing stations, but no engineer would think of introducing for new works machines of the coreless type. The drift of practice has been towards making the alternator in all respects satisfactory from an engineering point of view. It must be, first and foremost, a *machine*, and a machine that will run continuously without giving trouble. There is no

doubt that iron-cored alternators can be constructed more in accordance with the principles of sound engineering than can those having armatures without iron, in which the coils, long and thin, are supported in a not very mechanical manner at one end only. Engineers have come to regard such machines as unsuitable for prolonged hard work, and especially is this the case when the armature is stationary. Under the latter condition, soundness in mechanical construction is very difficult of attainment. The fact is that the insulating materials, micanite, fibre, stabilit, ebonite, slate, etc., upon the strength of which the durability of such machines depends, are quite unfit to endure stress or to transmit power. In up-to-date alternators there is no force transmitted through the armature conductors, nor is there any mechanical stress to speak of on the insulating materials.

It used to be thought that coreless armatures had no self-induction, while cored armatures possessed this particular characteristic in great degree. The idea was based on erroneous conceptions of the magnetic circuit and armature reactions. Now we know that essentially there is no electrical difference between machines with iron cores and those without, and that with proper designing there is no difference with respect to armature reactions. But, it may be remarked, the coreless machine was wholly unfitted for polyphase work.

Of the cored class, armatures with well-defined poles, such as were used by Ganz and others, have disappeared. Perhaps no firm has done more or better work than the Buda-Pesth firm, and up to 1896 their alternators did not sensibly differ in design from the machines they installed at Rome in 1885. But at the Paris Exhibition this design was missing, and really it was not before time. In the machines referred to, the resistance of the magnetic circuits varied greatly for different positions of the magnets relatively to the armature coils. When the poles were in line with the coils the air-gap was comparatively small, and the induction path was mostly through iron. When the poles occupied a position midway between the coils, however, the magnetic resistance was comparatively high, and the inductive path was largely through air. The result was loss of power in field hysteresis and a great amount of noise, while the flux through the magnets varied

between maximum and minimum, with a frequency equal to twice that of the machine, and in the exciting circuit disturbing E.M.F.'s of the same frequency were set up, due to the flux changes. By the thorough lamination of every part of the magnets as well as of the armature it would appear, however, that this variation in the field flux did but little harm, since the alternator in its day had an efficiency probably second to none of the Continental machines.

After the Frankfort Exhibition, Herr Coerper, of the Helios Company, modified the Ganz machine by providing the radial inwardly projecting cores of the armature with extensions, thus spreading the iron out in front of the coils, so that when the sectors, of which the core was made up, were all in place the surface presented to the field magnets was interrupted only by narrow spaces. It will be seen that this was a considerable improvement on the Ganz machine in the sense that the hysteresis loss in the magnets was reduced; the exciting flux was not subject to such large variation, and the exciting current was in consequence comparatively steady.

In the beginning of 1894 Mr. Kapp patented a modification of the Coerper machine, the improvement consisting in increasing the mass of the iron in the core sectors and enlarging the area of the joints between the sectors, thus making the core less discontinuous and reducing to a minimum the spaces left in the core for the accommodation of the coils. In all these machines the number of armature sectors was equal to the number of field-poles, the armature coils enveloping the separate sectors, and in Coerper and Kapp's machines being sunk into grooves in the sides. But it will be obvious that in all, the magnetic flux varied, with the relative position of the poles, though in the last-named machine to a very much less extent than in the first. When the field-poles were opposite the armature coils, the path of the induction from pole to pole included a joint between a couple of sectors, while on the other hand when the poles were in an intermediate position no joint was included. There was, therefore, though it might be small, a distinct difference in the reluctance of the magnetic path for different positions, and the variation of the magnetic flux due to this must have set up to some extent wasteful parasitic currents *in the mass of* the field magnets. The author

endeavoured to get rid of the imperfection above referred to by reducing the number of sectors and armature coils to one-half the number of field-poles. In his 1895 design each coil was embedded in the centre of a sector, being wound through two holes, and no part of the coil being near the joints the core sectors could be butted together, thus securing what was practically magnetic continuity for the core. With half the number of joints, even if they were imperfect, there was little danger of their producing a variation of flux through the magnets, since, whatever the position of the poles relatively to the armature coils, there was always a clear path through jointless iron for all the lines from pole to pole. This being so, hysteresis loss was avoided and lamination of the poles throughout rendered unnecessary. Many of these machines are at work, but they are not made now. The sector form of armature is costly to construct, and it offers no advantages to speak of, while it is ill-fitted for polyphase work.

These several designs illustrate well the evolution of the alternator in recent times. At first the armature had well-defined poles, and there was great magnetic discontinuity in the built-up core. The poles were then spread out at their extremities until they presented a nearly unbroken surface to the magnets, and there was in consequence some approach to continuity. Then the armature was filled up with iron, only such space being left as was required for the conductors. Finally the sectors were reduced to half the number of poles, in order to get what was equivalent, so far as magnetic flux was concerned, to perfect continuity. From this to the modern form of machine, with its armature core built up solid and constructed of interleaved segments pierced with holes, is but one step further in the evolution.

The field magnet introduced by Mr. C. E. L. Brown, excited by a single coil, is a thing of the past, the inherent defects of this type having forced it into disuse. This design undoubtedly showed a considerable saving in the copper required for the fields, with corresponding economy in the energy required for excitement, but the disadvantageous disposition of the field-coil and excessive drop in volts from no-load to full-load, when working on a circuit even moderately inductive, rendered machines with such fields unsatisfactory, especially for transmission work.

Added to this there was, owing to the magnitude of the stray field, difficulty in predetermining the path of the useful flux with accuracy, and predictions of the output could not be made with certainity.

In another form of field excited by a single coil, the poles projecting from the central core instead of being bent over to present a series of north and south faces lying in one circle were cut off, so that they formed a crown of north poles on one side and south poles on the other, the armature core being divided into two parts to correspond. This design allows of the field-coil being stationary, and to machines so constructed was given—not with much reason sometimes—the name "inductor-alternator." This type of generator attracted considerable attention on the Continent some five or six years ago, and much was hoped from it. Herr Dolivo-Dobrowolsky, the chief engineer of the Allgemeine Company, took it up with zeal in the belief that it would come to the front and stay there; but, alas! it has gone with many others. At the Paris Exhibition there were only one or two such designs, and when the members visited Germany none were in hand. The machines which were put into use some time ago are, of course, running and doing good work, but as a standard the type has ceased to exist. The fact is that the total weight of material in these designs much exceeds the weight in those with multiple field coils, so that there is no real economy in construction. The necessarily small air-gap which had to be employed is unfavourable to the production of a purely sinuous E.M.F. curve, while they have been found unsuited for power work because of the disadvantageous position of the exciting coil, already referred to, and the induction of opposing E.M.F.'s in portions of the armature winding. The shaft and engine parts of such alternators are magnetised, which is another disadvantage, but, to crown all, the machine, so far as its field is concerned, is illustrative of the foolishness of putting all one's eggs into one basket. It is all very well for small machines, but the work involved in repairing a large alternator of this type, should a field-coil happen at any time to become *hors de combat*, can only be contemplated with positive dismay. In all designs the primary object should be to secure immunity from breakdown, but it is of scarcely less importance that should at

any time a breakdown occur the repair should be effected with the greatest facility.

It comes to this, then, as regards alternators, that taking all the divergent types which were the vogue, the lines of evolution proceeding from each as an origin have been, during the past decade, convergent towards one type. This type, which is common to America, the Continent, and this country, is a machine with rotating radial magnets energised by multiple coils, having a cored armature with a cylindrical surface presented to the fields, and the winding lying in holes or slots below, but close to, the surface of the iron. Of other types there are a few, and there will always be special designs to suit particular circumstances, but the above is the standard and, there is reason to believe, the permanent type. There are in use two forms of it, one with the armature encircling the poles, and one with the poles encircling the armature, but machines of the former class are greatly in the majority.

Conditions Affecting Design.

Though general agreement has been reached as to the type and form of generators, agreement with respect to proportions or details of construction has not yet been arrived at. This is due largely to the variety of conditions which designs have to fulfil, while every machine, of course, is stamped more or less conspicuously with the originality of the designer, representing, as it must, the results of his study and investigation. On the Continent they have gone in solid for low speeds, and there machines are invariably driven by vertical engines of the marine type or by horizontal engines, such as are made by Weyher and Richemond or Sulzer. The difference in the speed of engines of similar size made by different makers is inconsiderable, while the difference in speed between the smallest central station engine and the largest is not great. A 500 H.P. engine, for instance, will run at 120 revolutions per minute, while a 3,000 H.P. engine will run at 83 revolutions. In this country we have quite a different state of affairs, and while the high-speed engine is at present in general use, there are *really all sorts* running at all speeds. One never knows

here what size of generator has to be supplied for a specified output, and to the multitude and variety of engines is to be attributed, in large measure, the little progress that has been made in the standardisation of large machines. So long as we had only one high-speed engine to fit we got on very well; but now there are at least half a dozen, no two of which correspond for power, in speed. Not only so, but we have 2-crank and 3-crank engines by the same makers, the latter, seeing that each individual line of parts is much lighter, running at considerably higher speed than the former for the same power. And, as if all this were not enough, we have Mr. Mark Robinson making observations of periodic oscillation of fly-wheel systems, which cause his firm to demand that the armature body of the generator shall always form an inherent part of the engine fly-wheel, thus necessitating extra pattern making. On the Continent, so far as I can learn, the designer has none of this worry to contend with.

Owing to the general adoption of low speeds, the frequency of continuous-current machines is on the Continent lower than with us. Take, for instance, the continuous-current sets running at the Hanover Central Station, which have an output of 400 kilowatts. These machines are coupled to vertical engines running at 120 revolutions per minute, while machines of the same output would with us be coupled to high-speed engines running at 300 revolutions per minute. In the former the frequency is with 10 poles, 10; in the latter, with 6 poles, it is 15. Siemens & Halske's 1,000 k.w. machine at the Paris Exhibition running at 95 revolutions per minute had a frequency of 11; the Siemens Brothers' 1,500 k.w. machine, running at 200 revolutions, had a frequency of 26. Lahmeyer's 350 k.w. machine at the Paris Exhibition had a frequency of 9·4, while the large 1,500 H.P. machines at the Berlin Central Station run at 83 revolutions and have 16 poles, corresponding to a frequency of 11. From these figures it appears that on the Continent the usual frequency for continuous-current generators is from 9 to 11, while with us it is fully 50 per cent. higher. In passing, it is worth while noting that in machines of similar *power*, provided the core induction is the same, the hysteresis loss is practically *unaffected by frequency*, and the percentage of power

wasted in hysteresis is virtually settled by the induction in the iron. At first sight this appears somewhat strange, but it is capable of easy proof. After all it is only reasonable to suppose that there is some definite relation between the energy output of the machine with which the mass of iron is associated and the energy spent in the magnetic manipulation of the mass itself.

But in determining the output-coefficient of a given size of armature, the question of speed and frequency is of considerable importance. Calling D the diameter of the armature in inches, L its length in inches, and R the number of revolutions per minute, by output-coefficient is to be understood the value by which D^2 L R has to be multiplied to give the output of the machine. The machines at Hanover have been referred to, and the armatures of these may be taken to illustrate my meaning. Let us imagine that we increase the speed by 50 per cent., making them run at 180 revolutions per minute instead of 120. The frequency would then conform to English practice, being 15 instead of 10. Temperature is the factor which limits the output, as the proper course is to find for any given size of armature the load which corresponds to the maximum temperature rise permissible and then to rate it at that. Well, we find that if this temperature rise is not to exceed at the higher speed the figure attained at the lower, for a 50 per cent. speed increase the output cannot be increased by more than from 25 to 35 per cent. The exact figure is a matter for experiment and depends upon the construction of the armature. Even if we increase the depth of the core under the slots by 50 per cent. and assume that the added depth is effectively on a par with the original depth, which it is not, we shall still have an output-coefficient considerably reduced on the high-speed machine. This means that the output-coefficient of an armature depends upon the speed at which it runs and explains its much higher value in some of the slow-speed Continental machines. If I might use Mr. Mavor's term, the so-called active belt becomes more active as the speed is reduced. But, unlike Mr. Mavor, I find little agreement in the output-coefficient between the machines of different makers. I have compared a very large number, but I will not trouble you with the con-

stellation representing the results, as it indicates no law. While adhering to the conditions that the frequency shall be about 10, that the temperature rise after a six hours' run at full load shall not exceed 40° centigrade and that there shall be no sparking, there is possible, of course, great variation as regards the length of air-gap, the air-gap induction, the ampere bars on the armature per pole, the number of poles, and so on, but on the whole it is easier to design a satisfactory low-speed machine than a high-speed one.

Not very long ago the late Prof. Short, in a paper read before the Manchester Section, proposed a series of machines embracing sizes from 137 to 2,000 k.w., and all consistent as regards efficiency, temperature rise and sparking, and overload limits. This result was obtained by keeping the length of armature the same throughout the whole series and varying the diameter. The number of commutator sections and conductor bars was to be proportional to the diameter, and there was the same current per conductor path for all sizes. The number of poles was proportional to the diameter, while the speed was inversely as the diameter. The proposition was simplicity itself, but our present ideas respecting engine designing prohibit us from putting it into practice. The engine speeds, for instance, do not vary inversely as the power, nor does it appear in the nature of things that they should do so, though this is a condition which Mr. Short's proposition involves. If the engine of a machine for 1,000 k.w. has a speed of 95 revolutions per minute, the engine coupled to a 2,000 k.w. machine will not run at 48, nor will that coupled to a 3,000 k.w. run at 32. The speeds of these will be about 85 and 75 revolutions respectively, and the dynamos will have to be designed to suit. As a matter of fact, the speeds given by Mr. Short are for the smaller sizes too fast and for the larger sizes too slow; and much as we should like to do so, it is impossible, having regard to economy in engine design, to arrange that all the different sizes of dynamos shall be multiples of one particular unit. The extra expenditure involved in the construction of steam sets running at such unnecessarily slow speeds for the large sizes, as Mr. Short advises—53 revolutions for a 2,000 k.w. machine — would more than counterbalance any advantage gained from carrying out in design the multiple unit idea.

In alternators the frequency is fixed, but the design is considerably modified by attachment to high, instead of low, speed engines. Taking a 3-phase alternator of 750 k.w., the field of this machine mounted on the shaft of a low-speed engine, such as is met with everywhere on the Continent, would run at close on 94 revolutions per minute, and would measure, say, 18 feet across the pole-tips. Now a machine of the same output coupled to a high-speed engine would probably run at 200 revolutions per minute, and the diameter of its field would be 10 feet. This gives a periphery speed of over 6,000 feet per minute, and though this has been exceeded, I must say I think it quite high enough. Not only are the poles now heavier, but the tangential inertia per unit of mass is greatly increased, demanding a greater proportion of the fly-wheel rim to be drilled away for the larger bolts required. Such a fly-wheel would, of course, have to be made of steel, the rim and arms being solid and the boss split triangularly and steel-hooped. To get the output, the width of the machine must be half as much again, so that if the low-speed alternator were 12 inches, the high-speed one would be 18 inches, the proportion of width to diameter being therefore altogether different in the two designs. If the core induction were in both machines the same, in each the same quantity of heat would be produced, due to hysteresis loss, the mass of soft iron to magnetise being the same; but as the radiating surface has been in the smaller machine reduced by some 20 per cent., it is obvious that if the output-coefficient is to be maintained at the same figure in the high-speed machine as in the low, more efficient means of carrying off the heat must be provided. It is necessary to bear in mind all these facts when comparing the designs seen here with those seen abroad, and it does not do to conclude that what is good proportion for one case is good for another where the conditions are quite dissimilar.

CONTINUOUS-CURRENT MACHINES.

Returning to continuous-current generators, there is not a great deal of difference between the practice abroad and what we are accustomed to here. The number of poles for a given diameter of armature varies in different designs, but

generally speaking there is in large machines a pair of poles for every 15 to 18 inches diameter. Rarely do we find the current to exceed 300 amperes per set of brushes, which corresponds to 150 amperes per path in a parallel wound armature. The output varies from 100 to 150 kilowatts per pair of poles.

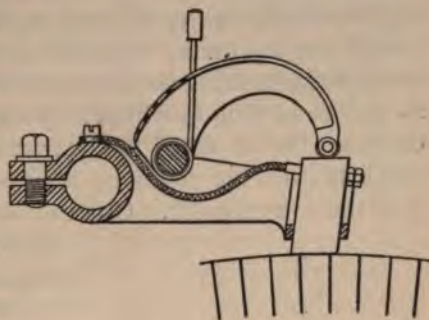


FIG. 1.

Carbon brushes are now universal, and a great deal of ingenuity has been expended on the effort to obtain a thoroughly satisfactory brush-holder. Nothing is of more importance in the design of a machine than the brush gear, and as illustrating the many different forms devised, perhaps no collection could surpass that at the recent Glasgow Exhibition, where all kinds were to be seen. Under three classes nearly all the brush-holders in use can be grouped, viz., the slider holder, the hinged holder, and the reaction holder. In the former the carbon slides in a rectangular slot, being pressed directly on the commutator by a spring, or a toe operated by the spring; in the next, a carbon is firmly secured to a hinged arm on which pressure is put by a spring; while in the last the carbon is in the form of a triangular wedge, one side of which is kept up to the commutator by the force resulting from a spring pressing on another side and the reaction of the brush-holder surface, against which rests the third side of the triangle. These different forms are represented in the sketches, and all give satisfactory *results, but of* the three the slider form seems

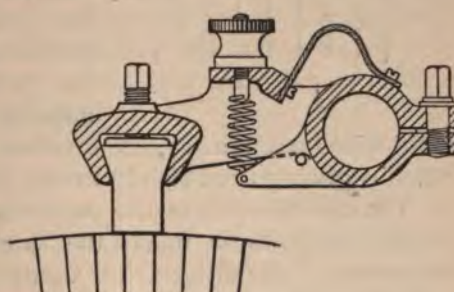


FIG. 2.

the most in favour. Whatever is used, it is necessary to have the moving parts as light as possible in order that the brush may promptly respond to infinitesimal irregularities in the commutator; some makers using the second class of brush-holder construct the hinged arm of aluminium for the sake of lightness. That the pins or brackets carrying the brush-holders must be absolutely rigid goes without saying, as well as the ring to which they are attached. The fixed ring carrying the whole of the gear may be supported either by brackets from the yokes, or may be attached to the pedestal next the commutator. One method is just as good as the other, but the former must be employed when the armature is fixed in the middle of the crank-shaft between the engine cylinders, while the other is the more suitable for machines of medium size coupled to one end of the engine shaft. The construction in the latter case is somewhat simpler than when the bracket supports from the yokes are adopted, but personally I should like to see the whole paraphernalia of adjustable ring, screw gear and hand wheel swept away, the brush-holders, after testing the machine, being fixed rigidly in one position to suit all loads.

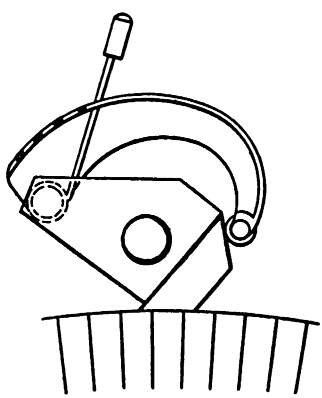


FIG. 3.

The construction of the poles and yokes in the Continental machines is much the same as with us. In the machines of the Allgemeine Company and Schuckert the magnet cores are of solid steel cast in one with the yokes. The former company fit on to each magnet a pole-shoe, which is made by cutting into segments a steel ring turned inside and out, and this shoe is fastened to the magnet by screws. The Union Company of Berlin cast the steel magnet and polar extension in one, and screw the same on to a yoke of steel or cast-iron as the case may be. The eternal question of steel deliveries is as pressing on the Continent as with us, and at the time of the Institution visit the Allgemeine Company was settling it by adopting cast-iron for their yokes instead of steel, and using laminated

blocks for their magnets. The latter were being made of thin plates riveted together and screwed on to the yokes, being drilled and tapped for the screws just as if they were solid. The plan of casting the laminated blocks into the yokes does not seem to have been adopted by any makers on the Continent. The experience of my firm is that a laminated pole offers no advantages as regards efficiency, while it has several disadvantages, not the least amongst which is the increased tendency to bucking observable with their use. There is less work in a machine having solid steel poles cast with extensions screwed on to a cast-iron or steel yoke, and the benefit derived from the adoption of the circular section in saving field copper is considerable. German steel is, I find, inferior to English steel, but it is much cheaper in proportion, the result being that even when the extra field copper is taken into account a machine made with German steel is cheaper than one made with English. On the Continent, as with us, the usual ratio of pole arc to pole pitch is ·66–·7.

The machines inspected in the Berlin station on the occasion of the Institution's visit excited universal admiration. These continuous-current sets are capable of giving each 1,500 E.H.P. at 250 to 280 volts, and are coupled one on each end of the crank-shaft of a 3,000 H.P. vertical engine running at 83 revolutions per minute. Each machine has 16 poles, and the armatures look about 12 ft. in diameter. The air-gap is about $\frac{3}{4}$ in., and the induction in the gap is 8,000 C.G.S. units per square centimetre. The plan of mounting continuous armatures directly on the crank-shaft between the engine cylinders appears to be viewed with disfavour on the Continent. Alternators there are in plenty with their rotating fields so placed, but generally speaking continuous-current machines are coupled on one or both ends of the crank-shaft. Coupling two generators to one engine permits of earthing the middle wire of the three-wire system without inconveniences due to rise of pressure on one side should the outer of the other become accidentally earthed. Lahmeyer, it will be remembered, secures similar immunity by connecting the earthed neutral wire to the middle point of the generator armature.

Visitors to the works of the A. E. G. may have observed that in the smaller machines the strips con-

necting the commutator segments with the armature windings were made of nickeline for the purpose of introducing resistances into the two short-circuited coils, a row of small holes being pierced in the strips for the double purpose of further increasing the resistance and ensuring rapid cooling. The practice of this company differs from that of other manufacturers in that the slots in the armatures of their dynamos are much smaller and the current density in the conductors correspondingly greater, reaching in some cases as much as 3,600 amperes per square inch of section. On the Continent there are no commutator end clamping rings in the larger machines, but in lieu thereof a number of clamping pieces, each secured by one or two studs, the pieces being made by cutting the ring, turned first to the proper wedge shape, into short pieces. It will be readily understood that this gives much greater facility for replacing segments than if a continuous ring were used.

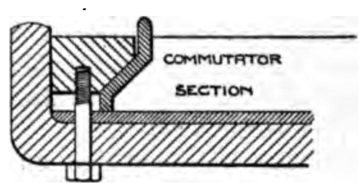


FIG. 4.

An interesting variation is afforded by the practice of Messrs. Brown, Boveri & Co., who make the commutator drum with a flanged end and tighten the segments by wedges placed circumferentially on the drum and pulled down radially. (See sketch.)

ALTERNATING-CURRENT GENERATORS.

Dealing with alternators, there was in single-phase machines nothing to speak of in Paris or Berlin, and it is unlikely that, save for extensions to existing stations, there will be further single-phase machines installed in this country. I have already referred to the design which has been universally adopted for polyphase alternators, and will now touch on some of the variations. The plates constituting the armature core are contained in a massive cast-iron circular frame, which has to be made very stiff in order to resist deformation, and several devices have been adopted with a view to getting maximum stability with minimum weight of material. Messrs. Brown, Boveri

secure to each side of a cylindrical shell a wheel-casting with arms extending to central hub or boss, this latter being bored out to fit on trunnion rings cast concentric with the bearings. This construction converts the whole armature into a wheel supported on a couple of trunnions having two sets of arms, between which the flywheel rotates inside a cage, so to speak. The construction is, of course, very rigid, besides admitting of the armature being rotated on the trunnions, and provision so made for examination of any of the coils needing attention. Messrs. Lahmeyer, of Frankfort, in building their armature frames also attach to each side of the containing shell, wheel-castings. They have arms like the Brown-Boveri construction, but these do not terminate in a boss bored out to rest on a trunnion. Generally they terminate in a ring just large enough to properly clear the shaft and brush gear. The 100 k.w. machine at the Paris Exhibition, for instance, had frames of this kind with ten arms in each casting. On occasions, however, the inner rings of these wheels are considerably larger than are just required to give clearance for the brush gear. In fact they may be half the diameter of the armature or more, in which case the castings, still consisting of concentric rings connected by radial bars, merely serve to increase the depth of the shell, and so give it additional stiffness. Messrs. Schuckert have endeavoured to stiffen the armature frame by connecting by means of radial stretcher rods the cast-iron shell at different points to a ring concentric with the shaft. It will probably be remembered that the 850 k.w. machine at the Paris Exhibition was stayed in this manner, the ring being made large enough to properly clear the brush gear for the field magnets. In this case the weight of the armature, instead of being as in the construction of Messrs. Brown, Boveri, taken by trunnions, is borne by the feet cast at opposite sides of the shell. Several designers, however, ignore these methods, and trust to the stability which can be obtained from making the section of the shell of box form or of a deep **U** shape. It is worthy of note that the largest alternators seen during the tour in Germany had no devices of the kind described. I am referring now to the machines at the Moabit station of the Berlin Electricity Works, giving 3,000 k.w. *at 83 revs. per minute.* These, if not the largest

generators in the world, were, up to the date of the Institution visit, the largest constructed in Europe. Matters may be so arranged that the lower half of the armature receives additional support from a pedestal placed directly underneath the shell and resting on the foundations. All the devices just mentioned are for the stiffening of a shell containing the core plates, but by far the most interesting design in armature frames was that presented by the departure made recently by the A. E. G.—the makers, by the way, of the large alternators above referred to—who proposed to do away with the cast-iron shell altogether. For this are substituted a couple of side-rings to clamp the core plates up, and from the latter radial struts project, which are connected together by tie rods, the whole forming a lightly-built framework of sufficient strength to support the armature core, and of a sufficient stability to resist deformation. A machine built on these lines, giving 1,100 k.w. at 107 revolutions per minute, was seen working on probation; others are being installed at Manchester, and, if successful, the construction will constitute a distinct advance. Mr. Dobrowolsky asserts that as compared with the usual design the saving in weight of materials required in respect of a given output will be about 20 per cent., and the saving in cost of construction about 10 per cent.

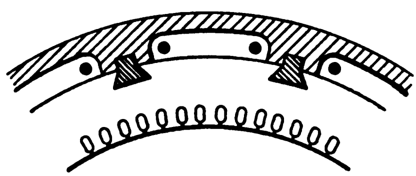


FIG. 5.

In modern alternators the segmental core plates are clamped tightly together by stiff outer rings, the tightening bolts in most cases going right through the segments and providing the means of securing these in the containing shell. When firmly clamped up the rings are secured in their place by screws passing through the shell, and it is now unusual to insulate the through bolts in armatures of this construction. The holes are punched close to the outside edge of the segment, say within one-eighth or three-sixteenths of an inch, and it appears that there is very little loss of power due to the field straying in this portion of the core to produce parasitic currents in the bolts. Fischer-Hinnen, in investigating this matter theoretically,

was led to the conclusion that the loss was quite negligible, and Boucherot, taking examples from actual practice, found that so long as the induction in the armature iron did not exceed 10,000 to 12,000 C.G.S. the loss in the bolts was not sufficient to make it worth while insulating them. Some years ago the majority of Swiss engineers had ceased to insulate the bolts, and now the omission is universal. It must be remembered that the system of shell, clamping rings, and tightening bolts, complete, form a huge squirrel cage, an amortisseur in fact, the current in which tends to blow the field out of its domain.

The Westinghouse Company do not use for securing the segments bolts going through them, but dovetail-key them into the shell, and the Union Company, of Berlin, adopt the same plan. The sketch shows the arrangement used by the latter company. The flange cast on the shell forms one clamping ring, there being substituted for the

FIG. 6.

other a number of loose plates tightened up by through bolts, 2 bolts between each key. The keys sunk into the shell are kept in position by radial screws, and in the front are dovetailed to receive the plates. It should be noted that this construction permits of the building of large armatures without the use of large machine tools. The shell being laid on the floor, the feet and the joints can be slotted by portable tools, and in like manner the slots for the keys can be machined.

The armature winding of alternators nowadays consists of coils wound through holes *in situ*, or of coils former-wound and laid in open slots. Most European machines are wound according to the former method, and most American machines according to the latter. In the Paris Exhibition, though there were fine examples of both, the hole-wound certainly predominated. In plates punched

with holes it is usual to make, in addition, a cut of about $\frac{1}{8}$ in. wide, so taking away the iron at the thinnest part in order to reduce leakage, while generally in slotted plates a small notch is made on each side of the slot to admit of a wooden spline being driven under the coils to keep them in place. The sketches show the punching in the two cases. The former-wound coil can be readily replaced in the case of a burn-out, and this is of great importance in power installations when the machines are particularly liable to damage, from lightning striking the overhead lines, and where repairs have to be effected in the shortest time by a comparatively unskilled person. It has been claimed that a hole-wound coil can be replaced as quickly as a former-wound coil, but this is by no means the case unless the coil consists of a very few turns. When we come to a solid bar in each slot with end connectors, whether these

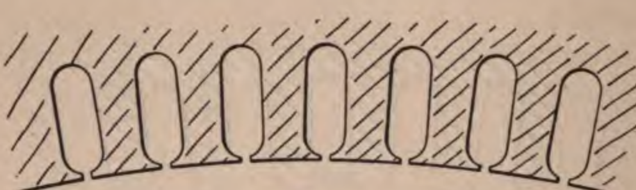


FIG. 7.

are laid in holes or slots makes no difference as regards repairs, and in such case the holes are preferable. But for machines of from 250 to 500 k.w., running at 3,000 to 5,000 volts, where the coils have many turns, the removable coils have far and away the advantage as to the time taken to effect repairs. If the coils themselves are to be as good in the replaceable form as when wound *in situ*, there is very little saving in the cost of winding. As a matter of fact they cannot then be former-wound in the same sense that a magnet coil is former-wound except for low pressure, as to make a thorough job it is necessary to wind the wires in seamless micanite tubes. Frequently we utilise at Charlton a section of the core for the coil-winding, placing the tubes in the slots, 4 or 6, as the case may be, and after winding simply knocking the coil, with the tubes on it, out of the slots.

The smallest number of slots or holes employed per pole

per phase is two, but as a rule the winding is distributed over as many slots as possible, 3 and 4 being frequently used. The spreading out of the winding reduces variation in the distribution of the magnetic field through the magnets to a minimum, while it ensures that the curve of E.M.F. shall approximate to the sine form—the ideal for all alternators. The large machines in the Moabit Station already referred to had a bar winding, there being 5 bars per pole per phase. In these the interior diameter of the armature was about 24 feet, and the current was generated at a pressure of 6,000 volts.

In three-phase machines the armature coils have necessarily to be interlaced, and the ends must be shaped in such manner that those belonging to different phases are kept well apart. There are two methods of arranging the coils which, for want of better terms, I call three-form coiling

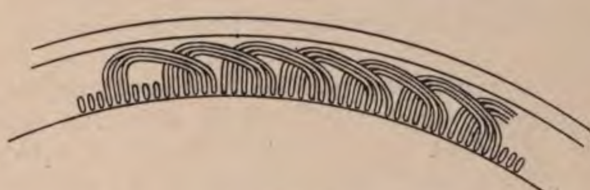


FIG. 8.

and two-form coiling respectively, and examples of both were to be seen in plenty at the Paris Exhibition. In the former there are three shapes of coil in a machine, viz., coils wound straight, coils with ends bent inwards towards the shaft, and coils with ends bent outwards. All the coils of one shape, constituting one third of the total number on the armature, belong to one phase, so that for the three phases the coils are straight, bent inwards, and bent outwards respectively. In the two-form coiling there are only two shapes of coils, those wound straight and those with their ends bent outwards, so that in each phase, constituting one third of the total, there is an equal number of straight and bent coils. In the three-form coiling, therefore, the coil ends lie in three tiers, in the two-form coiling they lie in two tiers. The latter is the more frequently used and is the simpler. When employing it there is on the armature only half the number of coils—one coil per phase to

every pair of poles—with, of course, half the number of crossings that there is in the three-form coiling, while the inwardly projecting ends of the latter are a good deal in the way.

Possibly visitors to the A. E. G. Works observed a new method of coiling for three-phase machines, in which the coil ends were twisted, but all of one form. It was intended for pressures above 3,000 volts, and the object was to get a winding in which the coil ends could be better insulated while the distance between adjacent coils, belonging to different phases, was kept nearly uniform throughout. The sketch gives a rough idea of the plan, and the winding, which is done *in situ*, is easy until the last coil is reached, when on account of the wire having to be threaded through between coils wound on each side, it is not at all easy. As Mr. Dobrowolsky naively remarked to me, a coil which breaks down is always from the winding point of view the last coil, so I am not sure that this new winding will come into use.

On the Continent the practice as regards field-magnet design, is pretty much the same as with us. In some machines the field pole and shoe consist of a block of laminated iron riveted together, which is dovetail-keyed into the fly-wheel rim. The sketch shows the method the A. E. G. adopt for keying in such blocks in the large machines at Moabit, but here the yoke ring into which the magnets are fixed is of laminated iron. The general practice is, however, to make the pole in steel with a shoe of laminated iron cast into it, a plan employed for some nine or ten years now by the Swiss engineers. Some designers are under the impression that laminated pole shoes are unnecessary when holes or partially closed slots are employed for the armature winding, and accordingly in their machines the magnet pole and shoe are made solid. My experience has led me to an opposite conclusion. Unfortunately we cannot always determine with accuracy the advantage derived from this or that modification when other slight modifications are introduced concurrently, but the results I have from time to time obtained, lead me to believe that the loss of power at full load is from 4 to 5 per cent. more with unlaminated than with laminated shoes. It must be remembered that in the allocation of the various losses in a steam alternator,

there is much that is guesswork. Suppose that perfectly reliable cards of the I.H.P. can be obtained at quarter load, it is very easy to calculate what addition should be made in respect of hysteresis and copper losses for full load. But in indicating the engines at full load we find this sometimes largely exceeded, and the difference has to be divided in some proportion between the engine and the alternator. This is where the scientific use of the imagination comes in. Undoubtedly, when it has been possible to test alternators coupled to engines, the friction law of which has been known, a very considerable loss has been shown to take place with solid poles, unaccounted for by calculation and increasing with the load. This loss may at full load amount to several times the hysteresis and eddy-current losses at no load, and it can only be due to parasitic currents in the poles. It can be almost eliminated, however, by proper lamination; of the pole shoe in any case; and of as much of the body of the pole as possible. In the Westinghouse alternators the poles are laminated throughout.

The action of the amortisseur or damping cage is well known, and a convenient way of providing for fitting it on laminated poles, is furnished by punching in the plates making up the block several holes across the pole face and close to the edge of the iron. The sketch shows five such holes, this being the arrangement adopted by the A. E. G. for all their alternators. Should circumstances arise when machines are set to work, rendering the amortisseur desirable, copper bars can be driven into the holes and joined by straps at the side to form the same. If no such circumstances arise the amortisseur bars are omitted, the holes being beneficial rather than otherwise, inasmuch as they ensure that the pole shoe is well saturated.

Some time ago I observed at a meeting of the Institution that British engineers had been putting too much material into their alternators, and I repeat this as a clear and definite statement of fact. For some reason or other, designers here had been for years the slaves of the idea that in an alternator only a very small drop in pressure between no load and full load was permissible. Trade representations that this or that alternator had a drop of only 2½ per cent. induced most designers to aim at a small drop regard-

less of the weight of material necessary to achieve the end in view, with the result, of course, that our neighbours on the Continent, who showed much better judgment in adjusting their designs to the end to be served, secured a great deal of the work. It is to be hoped that my colleagues are now mending their ways in this respect, and designing machines as do their competitors, to give say 5 or 6 per cent. drop at the full volt-ampere output with a power-factor = 1, and 30 to 35 per cent. drop with a power-factor = 0. This corresponds to from 13 to 16 per cent. for a power-factor = ·8. Incidentally it may be mentioned that a given machine carcase will give wound for two-phase just the same output as wound for three-phase with about the same drop.

Though within the limits assigned for temperature rise and efficiency, considerable latitude is allowed to the designer, I find a most remarkable agreement with respect to the value of the output-coefficient for alternators by the leading Continental makers. Taking a large number of three-phase machines, this is found to have an average value of ·015, all the fields working with an induction in the air-gap of from 7,500 to 8,000 C.G.S. High gap-induction and high field-saturation are both necessary to good design, but there are of course practical limits which have to be set to each. The stronger the main field, the less distortion will be produced in it by the armature field, and consequently the less will be the voltage drop.

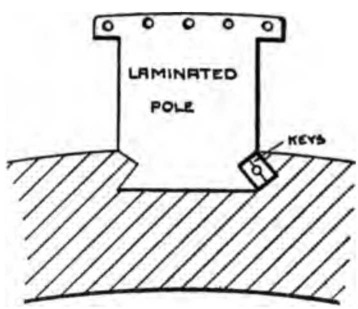


FIG. 9.

It is not my intention to trouble you at present with an investigation of the best relation of copper to iron, but it will be obvious of course that having regard to temperature rise, efficiency drop and price, there must be a *best* weight for the copper relatively to the other parts of the machine. Probably it would be found that for the armature this weight would be from 2 to 2½ per cent. of the total, and for the revolving field about 10 per cent. of the total.

TRANSFORMERS.

As in generators we have apparently settled down to one general form, so in transformers there is a tendency towards one common type. In this country the core form introduced by Messrs. Johnson & Phillips many years ago is most frequently seen, while on the Continent it is the type universally used. The peculiarity of the core transformer as distinguished from the shell transformer is that the coils are outside the iron, being slipped over the core. In the shell transformer the coils are surrounded by iron, and the core is built up of plates slipped one by one through the coils. In America shell transformers are still largely used, though there are not so many made as formerly, and the advantages of the core, as compared with the shell type, especially for power work, are now being recognised. First, the coils are outside, and, the surface being exposed, they are kept cool while running. The secondary coils, which are the easier to insulate, are placed next the iron, and the primary, in which the insulation is of relatively greater importance, are outside. By this arrangement a position where the cooling effect is greatest is secured for the high-pressure coils, a position where it is less important for the low pressure, while the iron is placed where its heating can do no harm. The transformer is therefore from the working point of view all that it should be, while the shell type is quite the reverse. Next to efficiency in working comes the question of repairs, and any one who has attempted these to both types will have no hesitation in deciding for the core. To take the iron to pieces and replace a coil in a shell transformer may be a matter of days, while in the core type slipping the yoke off and replacing a coil is a matter of hours. The flat rectangular coils used for the former are also much more trouble to rewind and more difficult of insulation than the coils used for the latter. In addition, the core transformer has a much smaller drop on an inductive load, which specially fits it for power-transmission work, which particular advantage, combined with the general advantages above mentioned, explains why the type is in universal use on the Continent. The plan adopted in America of *using for* three-phase work single-phase trans-

formers does not find favour on the Continent, three-phase transformers being always used. These, besides saving weight, and costing less to instal, give a better regulation where there is a likelihood of the phases being unbalanced in working. The position of the joints relatively to the coils is in core transformers a matter of importance. In three-wire single-phase working, for example, it is necessary if the two sides of the system are at all unbalanced to divide the secondary winding on both sides of the middle wire connection equally between the two limbs, otherwise good results cannot be obtained. As a matter of interest it may be observed that a transformer is always somewhat more efficient when the input is measured on the low-pressure side and the output on the high-pressure side than when the test is reversed, consequent on the eddy-current losses in the copper being in the latter case greater. From this it follows that a step-up transformer is always more efficient than a step-down one.

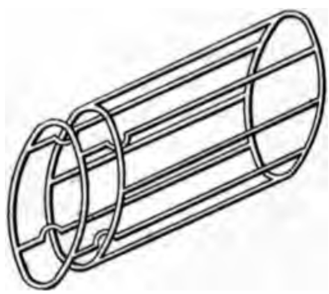


FIG. 10.

INDUCTION MOTORS.

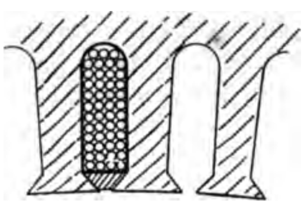
There is no specially novel feature to be recorded as regards the construction of induction motors. This subject was dealt with two years ago by Mr. Eborall in a masterly manner, and not much has occurred in the interim. I am not referring now to Mr. Heyland's latest machine which has been recently described in the Electrical Press, and which is now on its probation. Of this motor no doubt more will be heard presently; at the same time, as you are probably aware, though non-synchronous, it is not a purely induction motor. As will have been seen from Herr Lasche's paper, read in Glasgow, the A. E. G. are doing away with the outside cast-iron stator case in some of their motors, while in the particular motors used for the high-speed railway experiments the novelty is introduced of winding the rotor two-phase instead of three-phase. Whether it is intended to do away with outer

casings generally, as in the A. E. G. alternators already referred to, I am not in a position to say. At the Union Works in Berlin, curious squirrel-cage rotors for small motors might have been seen on the occasion of our visit there. Instead of having one set of bars as is usual attached to a single ring at each end, these rotors had all their bars joined at one end to a common ring, but at the other end alternate bars joined to separate rings, making virtually two sets of bars as sketch. I was puzzled to know the reason for this, but with the discovery of the designer, Dr. Neithammer, came the discovery of the reason.

In America the usual practice as regards the stator winding is to place former-wound coils in open slots; on the Continent the stator coils are always hand-wound through holes. Motors wound according to the latter method are superior from the running, but are inferior from the repair, point of view. The replacing by hand-winding of burnt-out coils in a large high-voltage motor where factory facilities are absent is a long and costly job compared with which the replacing of former-wound coils is easy. But of the superior qualities of the hand-wound machines viewed from the strictly electrical side, there can be no question, and some conversation I had with Dr. Neithammer illustrated this in a rather remarkable manner. The Union Works of Berlin started with American designs, and amongst others with open-slot stators for their induction motors. But by adopting, instead of the open slots, nearly closed slots—and, of course, hand coiling—both power-factor and efficiency have been increased, and weight and temperature-rise reduced. For small motors two methods of winding are in use, one by wire in slots, shaped as in sketch A, and one by sheet-copper strip in slots, and shaped as in B. In A the wire is not drawn through an insulating tube, but is pushed through the saw-cut at the bottom of the slot, a wood wedge being afterwards inserted under the coil to complete the insulation. Owing to the rather deep slot, the power-factor is a little lower than if there were no room required for the wedge, but the convenience of winding in this way is very great, and for low pressures the seamless insulating tube which would for high pressures prohibit its adoption is unnecessary. In B each *copper strip* is inserted through the saw cut at

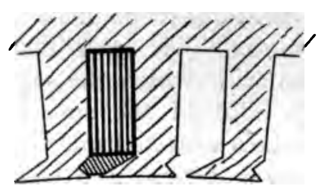
the bottom of the slot and pushed to the right till the slot is full, a wood wedge is then inserted under the group, and the strips in all the slots are joined up by end connectors to form complete coils. Owing to the ends of the strip coils being nearer to the iron, motors so wound have a power factor of 2 to 3 per cent. less than have those with wire-wound coils, but the strip-winding being less expensive and requiring a less skilled workman than does the wire-winding, the motors can be sold at a low price.

It is the practice on the Continent to wind all rotors above, say, 5 H.P., and to introduce in circuit with the winding resistances at starting. This is unlike the practice in the States, where motors of very large powers are made with squirrel-cage rotors. The latter are provided with auto-transformers, but for starting under full torque they



A.

FIG. 11.



B.

FIG. 12.

require to take from the line twice the normal working current, or the full working current for half torque. Seeing that there is a large number of cases in which high starting torque is not required, and where, consequently, it is unnecessary to provide for it, it seems scarcely in accordance with the proper adjustment of means to ends to draw a hard and fast line, and say above this or that horse-power all the rotors shall be wound. My experience of large motors with squirrel-cage rotors has been in every respect satisfactory, and I regard it as mere waste of money to provide for conditions which in practice can never possibly arise. The motor ought to have a short-circuited or wound rotor according to the conditions under which it has to work.

Conversion.

When it is necessary to convert polyphase into continuous current, the choice lies between rotary converters and motor generators. To the subject of sub-station equipment considerable attention has been recently given, but up to the present, discussion has only served to emphasise the fact that considerable difference of opinion still exists as regards the relative merits of the two conversion methods. The rotary converter has found much favour in the States, but though its birthplace was on the Continent it is there held in considerably less esteem, while here the opinions held regarding it are very varied. Most will agree with Mr. Steinmetz's statement that while rotaries are feasible at a frequency of 60, they are perfectly satisfactory only at 25; and from this it is clear that rotaries do not admit of the adoption of nearly so flexible a system as motor-generators. The latter allow of a frequency of 50, which means that in a general system for lighting and power, continuous current may be provided for the latter where needed, the alternating current being used throughout for incandescence and arc lamps and for power where continuous current may be dispensed with.

At the Mariannenstrasse sub-station in Berlin, members had an opportunity of seeing a most interesting installation of rotary converters working at a frequency of 50 and varying in size from 1,250 H.P. to 550 H.P. Here the behaviour of the machines appeared to be very satisfactory, not the least interesting feature being the method of regulating the pressure of supply. This is done on the high-pressure side of the machine by a special booster. The latter consists of a supplementary 3-phase armature through which flows the high-pressure current, the voltage of the latter being added to accordingly as more or less excitement is given to a magnet-wheel fixed on the converter shaft and rotating inside the supplementary armature referred to. The voltage of the low-pressure feeders is by these means completely under control, and the method of effecting the whole of the regulation on a small current undoubtedly presents great advantages over the plan of regulating the pressure by heavy current boosters on the low-pressure continuous side of the converters.

But the Mariannenstrasse installation only shows that 50 frequency is feasible for rotaries, not that it is to be recommended, and the difficulty experienced in the operation of these machines at this frequency is well known. Constructionally the rotary converter is of course a compromise. It is not a good alternator and it is not a good dynamo, while at all but the lowest frequencies it combines the vices of both and has the virtues of neither. When it is remembered that the usual frequency for continuous-current machines lies between 10 and 15, it will be realised that the difficulty of designing rotaries for 50 are great; and it should be recognised that for frequencies in the vicinity of 50, motor-generators are far preferable. Quoting Steinmetz again, "when properly handled and taken care of, under reasonably fair conditions the rotary at such a frequency will do its work well; but the more you become familiar with it, the less you are confident that you can rely upon it in any emergency, and that it will not go back on you when you need it most."

It appears then that for all but purely power conversions, and often then, the motor-generator will be adopted. Mr. Steinmetz's views I have given. Herr Dobrowolsky in Germany unhesitatingly pronounces in favour of motor-generators, and so does Mr. Brown in Switzerland. Mr. Hobart recommends that they supersede rotaries for any frequency above 25; indeed every engineer who has had experience with the working of the two varieties of plant, with whom I have talked, would prefer to use motor-generators.

As to difference in efficiency and cost between the two classes of machinery there is little to choose. In his Glasgow paper Mr. Field gave particulars of cost for both equipments, inclusive of all the accessory apparatus. With regard to efficiency, so far as can be ascertained from a mass of rather conflicting statements, the rotary has the advantage over the motor-generator to the extent of from 3 to 4 per cent. at full load, and from 4½ to 6 per cent. at half load. This appears to be the only point on which the rotary scores over the motor-generator; for parallel running, regulation and simplicity in working, the latter scores.

In writing the foregoing I had in mind synchronous

motors; I would like now to refer briefly to induction motors as applied to motor-generators. There seems to be an idea that induction or non-synchronous motor-generators are unsuitable for work of large magnitude. This cannot be on account of mere size, as motors up to 1,000 H.P. are working most successfully, and there is no reason why larger ones should not be built if wanted. Mechanically, induction motors are on a par with synchronous motors; they offer no difficulties in construction, while the driving of generators furnishes for them an ideal load. There is no doubt that the sub-station manipulation of non-synchronous motor-generators is simplicity itself as compared with that required for synchronous motor-generators, while in the former there is an entire absence of the tendency to "go back on you" which to a certain extent must characterise the working of all synchronous machinery. I have never met an engineer who does not prefer to work with non-synchronous rather than with synchronous plant, and the induction motor is so thoroughly satisfactory on all points as a machine, that every effort should be made to minimise the effect of, or cure, its one solitary electrical failing.

In the last sentence I refer, of course, to the idle current which must flow in the system due to the power-factor being less than unity, though this does not seem to have nearly such a pernicious effect as has been represented. In the first place, it must be remembered that motors running in a sub-station have a high power-factor as compared with motors of mixed sizes, some full and some partially loaded, running on a general polyphase system of supply. In the latter case the power-factor will be from ·6 to ·7, while in the former, owing to gradually improved design, it may be over ·9. In an article in the *Electrical World* of New York, Mr. Mershon asserts that recent requirements of a full load power-factor of ·92 have been met, while a power-factor as high as ·96 has been realised. Now it is a mistake to suppose that for a synchronous motor the power-factor is always—perhaps I should say is ever—unity. It depends upon the field excitement and upon the adjustment of the E.M.F. curve of the motors to that of the generators. I have found great difference in the power-factor of a motor accordingly as it is put on the Blackheath Supply Company's

mains or our own mains at Charlton, and in the synchronous motor of a motor-generator it may easily be below ·95. My contention is that with respect to synchronous *versus* non-synchronous motors for sub-stations, this matter has in some instances been decided too hastily, since reflection would have shown that the inherent simplicity of the non-synchronous motor, coupled with its extreme reliability, would have more than compensated for a possibly slightly lower power-factor.

The power-factor in the generating station may be higher than the power-factor of the motor-generators due to the condenser effect of underground mains. In all probability this fact will be demonstrated very clearly by and by in the case of various power schemes. In America a firm making two-phase motors supplies a couple of condensers as part and parcel of the equipment, these providing the wattless current for the motors, which consequently take a power current only from the mains. Well the underground mains are condensers ready to hand, and though they will not be able to supply all the wattless current to the motors, they may supply a good proportion of it. Suppose, for example, that 1,000 H.P. is transmitted to induction motors twenty miles away, at a pressure of 15,000 volts through a three-core cable. The wattless capacity current of the line will be about equal to the wattless induction current of the motor, and the station will consequently in this case supply power current only. In any case the capacity of the mains raises the power-factor, and if we suppose that the voltage-drop in a line having no capacity, with a power-factor of unity, were fifteen per cent, the additional drop due to the idle motor current would at the worst not exceed one and a half per cent. In a line with capacity this, as we have seen, is reduced. We cannot get rid of the capacity current; that we must have, and the best we can do is to arrange matters so that the generators do not have to supply it. Working synchronous motors in such a way as to achieve this unfortunately means increasing the "go back upon you" tendency, while induction motors take the capacity current naturally. It might pay to actually increase the capacity current for induction motors, provided the generators were not called upon to supply it. Notwithstanding

many failures, I have hopes that really reliable condensers for the purpose of supplementing capacity in such cases will yet be obtainable.

It is only in exceptional cases that real need for conversion arises. With our present equipment it is for street tramways imperative, as we have no alternating-current machinery quite suitable. But for general distribution for light and power it is unnecessary, as this can be effected equally well by polyphase currents. What is going on as regards alteration in the mode of supply in existing undertakings must be taken as no criterion, however, of what should be done were we to start *de novo*. Single-phase supply cannot be changed to 3-phase on account of mains already laid, though the change to 2-phase is easy. This change has already taken place at the Leeds, Sheffield and other stations, and no doubt many more will follow suit.

With these remarks I bring this somewhat lengthy paper to a conclusion. Referring to the last tour of the Institution, of German methods and German thoroughness much might be written, but the Visit Committee have dealt to some extent with these matters in their reports. I cannot finish, however, without acknowledging my indebtedness to Herr Dolivo-Dobrowolsky for the kindness I received at his hands while in Berlin. Every member who went on tour will acknowledge that in answering the countless questions put to him, Dobrowolsky showed his patience to be as inexhaustible as his genius.

NOTES ON THE MANUFACTURE OF LARGE DYNAMOS AND ALTERNATORS.

By ERNEST KILBURN SCOTT, Member.

Part I.—Dynamos.

In Part I. of the following paper the writer proposes to consider some of the principal details which go to make up the modern multipolar dynamo, and then, after giving data as to the sizes for given outputs, etc., to pass on to Part II., which deals in a somewhat similar manner with the design of multiphase alternators. As the ground to be covered is a very wide one, the writer hopes that readers will excuse the paper being rather disjointed.

GENERAL CONTOUR.

In these competitive days the introduction of pleasing curves into a dynamo machine is an important matter. The effect of beauty of form and finish was well exemplified at recent exhibitions where machines, otherwise well designed, failed to attract attention, because of their crude, inartistic lines, or from being badly finished and painted. In this matter of appearance the contour and construction of the yoke is perhaps the most important item. When multipolar machines were first introduced, the yoke was usually polygonal-shaped, and very ugly. Now it is the practice to make it circular, with well-rounded curves in cross-section; and it might be mentioned that the latter method has the advantage that the yoke can be swept up with a strickle and so save much pattern making. From the magnetic point of view cast steel is the best material to use, but the yoke may very well be of cast iron with the poles cast in (see Fig. 1). The extra section given by the cast iron is of advantage in giving greater stiffness, and where there is considerable variation in load, as in traction work, the magnetism of good cast iron responds more quickly than do the cheaper qualities of cast steel.[1]

[1] A kind of cast wrought iron made from melting scrap wrought iron with about 6 per cent. of aluminium to make it fluid has been much used in America, and one of the German firms employ a compound magnetic circuit made by heating wrought-iron slabs to redness and then running them round with cast iron to the exact contour required. When turned out of the sand no wrought iron is visible, and the tooling is about the same as for an ordinary casting.

It is now, however, becoming quite easy to obtain excellent magnet steel at reasonable prices and in good time, and on this account cast steel is being used for quite small machines, the poles being cast solid, with the yoke so that only pole-shoes are required to complete the circuit, as shown in Fig. 2. To remove a coil, only the pole-shoe needs to be taken off, which is much simpler than removing a heavy pole-piece. Absolute equality of material forming the magnetic circuit is one of the most important things to aim at, and for this reason the writer prefers having the poles solid with the yoke, the latter cast in *one* piece and afterwards cut through by cold saw or slotting machine to form the two halves.

Yokes are generally divided along the horizontal centre

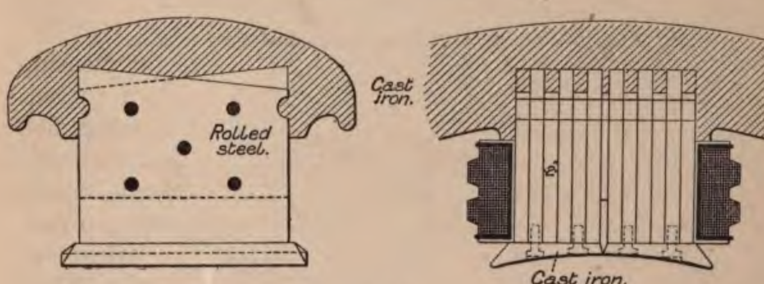


FIG. 1.

line, but some generators are divided vertically (the 1,800-k.w. Westinghouse machines at Bankside are so arranged) so that the two halves can then be withdrawn sideways for inspection of armature or field coils. There are advantages in this construction, and yet on the other hand the extra room required would appear to cut down the amount of plant which can be placed on a given ground space.

POLES.

Circular section-poles were introduced on some of the first multipolar machines made in this country, and are now much used because they give the shortest mean turn of field copper. (It is of course now generally recognised that the field-winding must be as close up to the nose of the pole as possible so as to reduce leakage and distortion of lines in the air-gap.) The modern tendency to reduce the number of armature

slots to as few as possible, calls for lamination of the pole surface to prevent undue eddy current loss. The bolting on of an entirely laminated pole is a somewhat awkward business, the method generally adopted, being to build

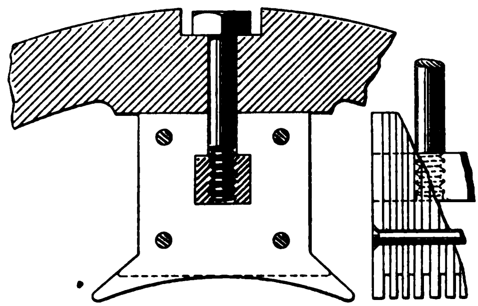


FIG. 3.—Method of Bolting Laminated Pole to Yoke.

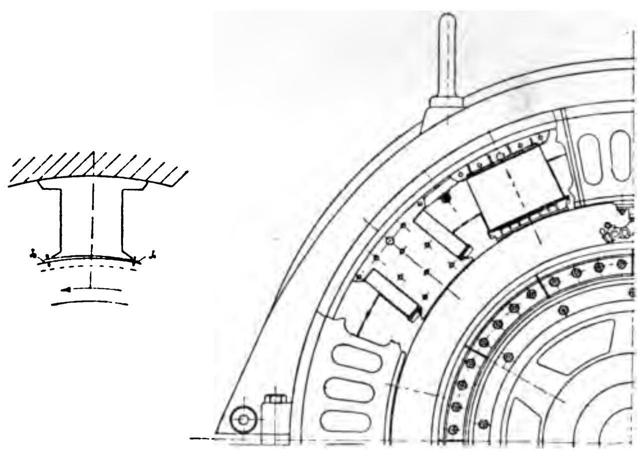
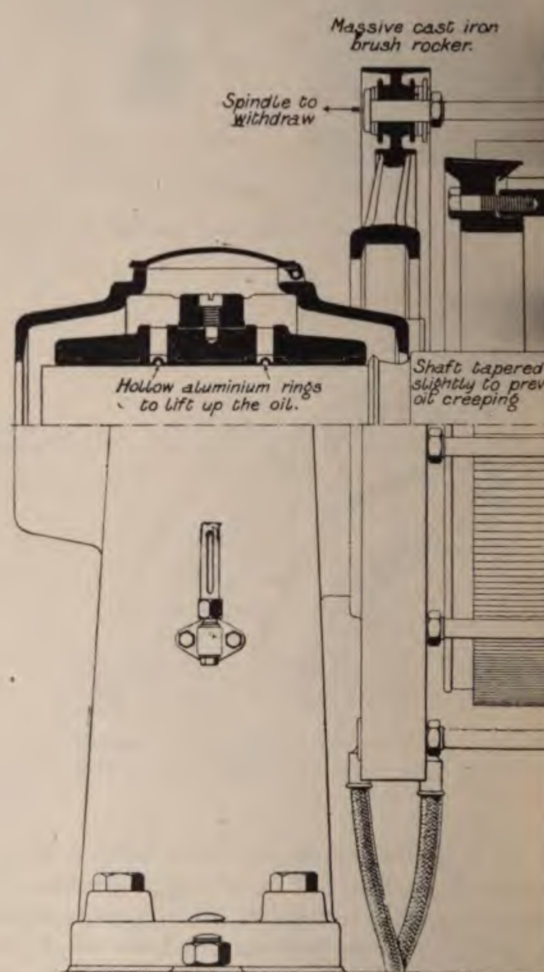
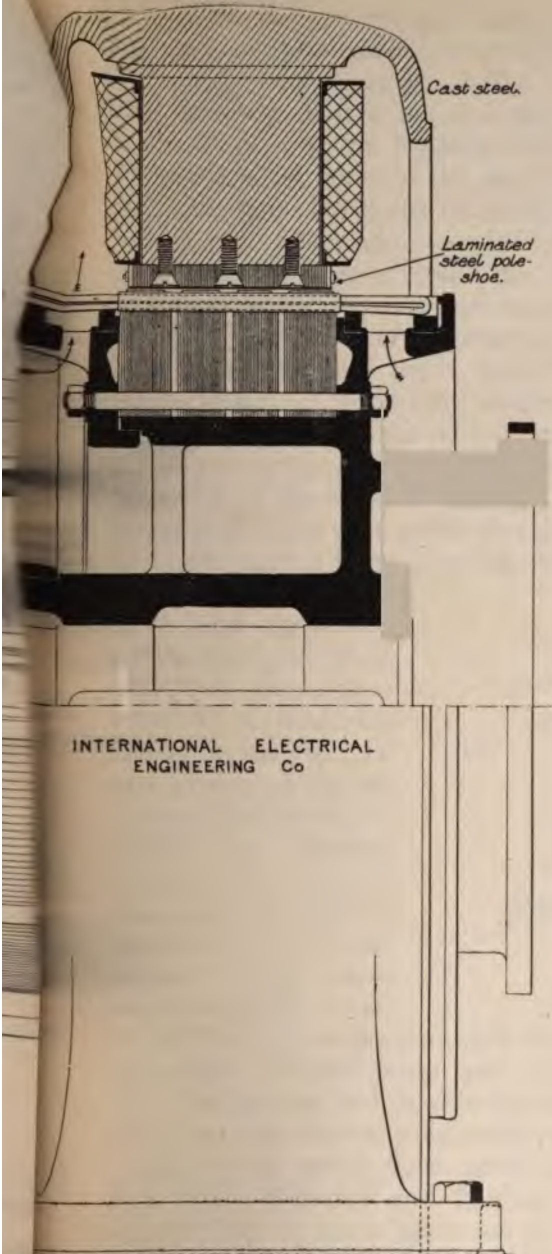


FIG. 4.—Sketch showing Method of attaching Poles to Yoke, also the unequal Air-gap of a Siemens and Halske Dynamo, 1,000 k.w., 550 volts, 95 revolutions.

up the poles round a solid steel bar, as shown in Fig. 3. The Oerlikon Co. cut a vee groove in the root of the pole, into which a double-shanked bolt is pressed. Siemens and Halske, of Vienna, have used the method shown in Fig. 4, the poles having projections where they bear against the inside of the yoke, being held in position by wedge-shaped packing pieces. As lamination is really only required at the


Massive cast iron brush rocker.
Spindle to withdraw
Hollow aluminium rings to lift up the oil.
Shaft tapered slightly to preve
oil creeping


Cast steel.
Laminated steel pole-shoe.
INTERNATIONAL ELECTRICAL ENGINEERING Co

to the centre, because in the middle of the pole the air-gap should be as short as possible.

Another object in cutting back the pole tips is to enable the armature coils to enter and leave the field by a gradual transition, and naturally if the reluctance under the horns is increased, the lines are compelled to distribute themselves more gradually. Probably the best method to insure the conductors leaving a field of force and entering another gradually, is to shape the polar surfaces diagonally, as shown in Fig. 8. Both Ganz and Schuckert have adopted this method. Some firms place a thin cast-iron liner or barrel right round the armature. At first sight one might think

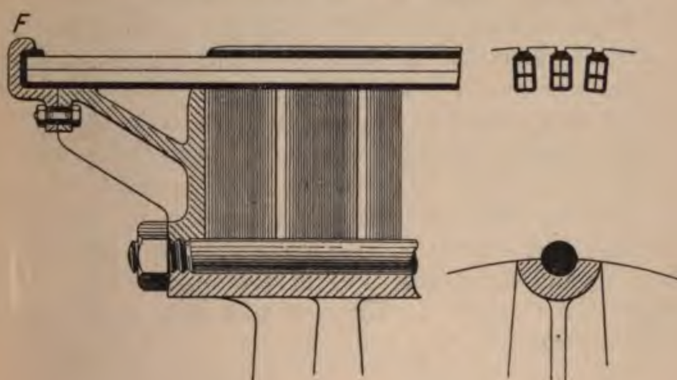


FIG. 9.

that this would short-circuit too many lines of force. It is found in practice, however, especially with the slotted form of armature, that the waste of lines of force is much more than covered by advantages gained. It gives a more equal distribution of magnetic lines in the air-gap; good support to the magnet coils; and certainty of the top half of the yoke coming down into its exact position after temporary removal. In order to keep the short-circuiting effect as low as possible, the liner should be relatively thin and made with a series of slots midway between the pole horns; the iron ribs being, of course, always saturated, leakage is not serious.

ARMATURE HUB.

A feature of recent armature hub design has been the transference *of the driving* stresses direct, instead of by

shaft and key. It is clear that whether the engine is of the high-speed vertical type, or the horizontal slow-speed with a heavy flywheel, it is well to transfer the turning effort to the armature-core and conductors in as direct a manner as possible. The old method of having a half coupling on the armature-shaft is, therefore, giving way to the design in Figs. 2 and 21, or in case of a dynamo built directly on the engine crankshaft, the hub is bolted up directly to the flywheel arms, as indicated in Fig. 22. In the same way it is now best practice to bolt or fix the commutator to the armature hub, and not key it separately to the shaft as heretofore. Relative movement between armature-conductors and commutator segments is thus less likely to occur.

For preventing shrinkage strains in casting the hub, the writer prefers the method shown in Fig. 22, it having the advantage over a solid boss and separated arms that the shrinking-rings assist in securing the hub to the shaft.

Rigid support should always be given to the ends of the armature-conductors by cast-iron cheeks bolted to the core, and if these castings are made as in Figs. 9, 19, and 21, the extra magnetic leakage is inappreciable. By a little care in arranging the arms, the hub may be made into a fairly efficient fan to draw air forward and pass it through the ventilating ducts.

The Armature Core.

Most armature discs are still stamped out by power presses, although the best work is undoubtedly done by cutting them off in a lathe fitted with bevelled cutting wheels, fed forward to a stop as shown in Fig. 10. Absolute concentricity of the holes and the periphery is thus ensured, and the burr can be taken off with a file as the disc revolves. By this means also any size armature disc can be made without incurring the great expense of punching tools. For example it is very convenient to be able to cut a few plates say one-eighth inch smaller in diameter for the binding wire to rest in. Again in slotted armatures, if the end core plates are unsupported, there is a tendency for them to bend over and give a very straggling appearance, besides being dangerous electrically. The best way to obviate this is to cut the stampings gradually smaller and smaller in diameter for about ¾ inch, so that the corner

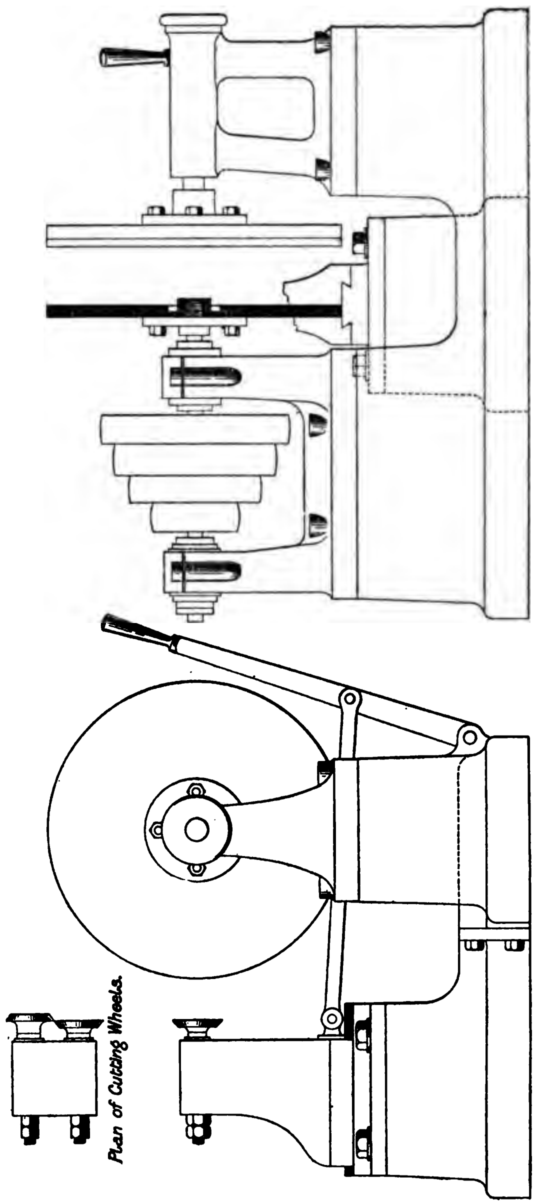


FIG. 10.—Machine for Cutting Armature Discs.

gradually rounds off to the bottom of the slot. When the plates are cut, this is easily arranged. Another reason for cutting armature core plates instead of punching them is that the latter tends to increase the hysteresis loss.

The point, at what diameter to leave off using complete discs and to begin building up in segments, is one upon which opinions are divided. The writer has known single discs to be used for armatures as large as five feet in diameter, and there is this to be said for such a practice, that the single discs are cheaper to assemble and secure.

Speaking generally, however, segments are preferable for armatures larger than say $3\frac{1}{2}$ feet in diameter, as large discs of the slotted type are liable to cockle in annealing, and set the teeth askew.

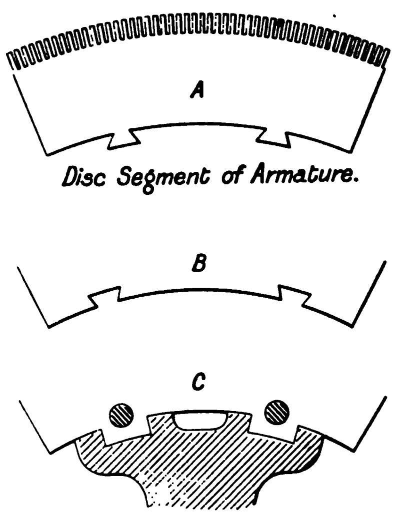


FIG. 11.—Methods of attaching Armature Segments to Hub.

Mild sheet steel, such as is used for armature stampings, is often slightly thicker in the middle than at the edge. Taken singly the difference is barely perceptible, but where many hundreds of plates are pressed together to form a core, the inequality will show itself unless care is taken in punching the discs so that when threaded on the shaft the inequality of one plate balances that of the next.

If core plates are in one piece they can be driven by steel keying bolts half in the core and half in the hub, as shown in Fig. 9, whereas segments must either be held by bolts right through the core, or else have dovetailed pieces on the inner periphery as shown at A in Fig. 11. Another method is to employ straight slots with bolts, as shown at C. American practice is to have the segments short and punch them out, teeth and all, at one blow, but on account of the labour of assembling it is just a question ther a long segment is not the best. Of course, a long

segment must necessarily be cut and held by bolts right through the core or else by vee notches on the inner periphery as shown at B in Fig. 11. Where the core plates are held by bolts it is usual to make the bolts fit tightly into the end castings. Mr. Ravenshaw has, however, adopted an endless chain construction for the 800-k.w. dynamos in the Leeds Tramway station, in that the bolts are rhymered to a tight fit in the core, but are made to clear where they pass through the end plates. The core is, therefore, like an endless chain, the bolts forming the pins of the links. Of course the core is screwed up tight, plate to plate, and is driven on the inner periphery by keys. A curious fact in connection with the wear of armature core plates is that they tend to get slack radially and not circumferentially.

Unless milled out from the solid, slots must of course be punched, and machines for this purpose have been developed which will punch as many as eight slots at a time, the feed being automatic. The burrs of the teeth are removed by an emery wheel. In some cases it is the practice to punch out the slots slightly small, and then, after building up, to clean out the grooves with a milling cutter.

Some manufacturers place great importance on the turning up of the laminated iron after it has been assembled, and this being so, not nearly so much care need be taken in punching out the plates or segments in the first instance. Slotted dynamo armatures are tackled by cutting the plates a little too large and punching out tunnels; the assembled core is then turned up, and finally the top of each tunnel is milled out and so converted into a slot. Such machining tends to burr the plates over and increase eddy current loss; the burrs may, however, be removed by treating the surface with a solution of sal-ammoniac. The passage of the air from the centre radially to the periphery of the armature is frequently provided for by inserting webbed gun-metal castings at intervals in the core. Another method is to rivet small washers to one of the side plates, whilst another consists in using a steel plate about $\frac{1}{16}$ inch, and punching small circular depressions in it say $\frac{1}{4}$ inch deep, these serving as distance pieces between the adjacent plates of the ventilating space. The teeth are held apart by punching projections in the periphery of this plate and turning *them half round.*

Insulation of Core Plates.

There is much difference of opinion as to how core discs should be insulated. Some advocate paper or japan, others are of opinion that the oxide coating which covers the steel during the annealing process is sufficient. It is quite certain that in time both paper and varnish tend to become powdery and drop out on account of the vibration and the constant heating and cooling, and the core loss is

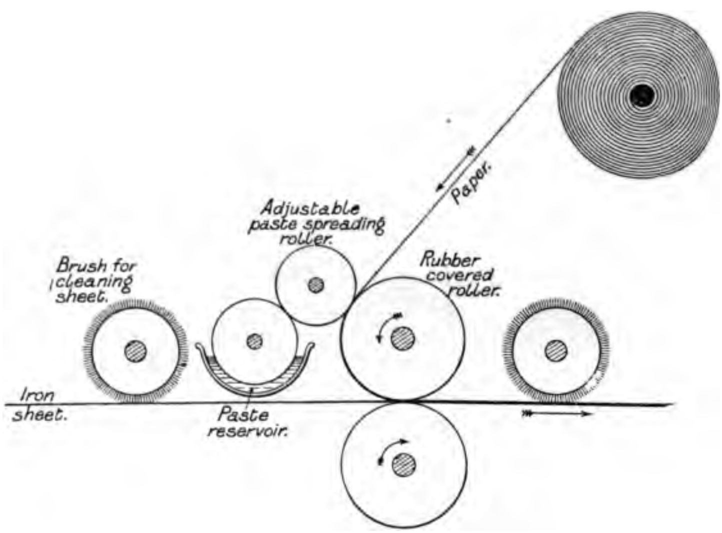


FIG. 12.—The Stolberg Paper pasting Machine.

thus liable to increase by the plates coming in contact. It has been suggested to paint the plates with a plumbago and water mixture, and then polish. Obviously the greater the nett sectional area the greater the output for a given sized carcase, and from this point of view, oxide coating is best. For plates ·014 inch thick the ratio of the nett iron section to the total cross section is about as follows :—

Insulated with paper	87	per cent.
„ „ japan	90	„
„ „ oxide	93	„

If varnish or japan is used it is advisable to dip the plates overhead in the japan and then pass them between

rubber rolls, as by this means the plates are evenly covered on both sides in one operation, and only half the total number need go through the rolls. Sometimes rollers are arranged so that the bottom one rotates in varnish, and so lays it on the plates as they go through. This is a rather cleaner operation, but it has the disadvantage that the top roller only receives its varnish from the bottom roller, and as it soon runs dry a large plate may not be evenly covered. When the plates are large they may be tackled by laying on the japan or varnish or paper pulp with a pneumatic sprayer. This method is particularly handy where there are a very large number of teeth, as the latter are very liable to be bent or torn off.

Where wood pulp paper is used as insulation the papers should not be threaded on separately, as it wastes much time. Sometimes the paper sheet is gummed to the plate, but this gives inequality of insulation, and the superfluous paper has to be burnt or punched out of the slots. The best practice is to paste the paper on to the sheet by the machine shown in Fig. 12. The paste is made from starch dissolved in cold water, boiling water being added, and the mixture stirred to a medium consistency. It is laid on the rough side of the paper.

SHAPE OF SLOT.

The best shape of slot or tunnel is not so much a question of electrical dimensions as of machining, insulating and holding the conductors in position against centrifugal force. Half-open slots, as A in Fig. 13, unfortunately cannot be used for dynamos when the usual barrel winding is employed. The writer, however, indicates below a method of getting over the difficulty. Slots B, C, D, etc., give the necessary opening space to enable solid conductors to be put in from the top. For small diameter armatures the usual practice is to have the slot parallel and to hold the conductors in position by ordinary piano wire binding carried in depressions in the core, care being taken not to solder the bands right across. In large machines, however, say of above 4 or 5 feet diameter, these bindings are not sufficiently mechanical. Sometimes broad bands of delta metal are employed, the ends being cottered together in a

neat manner. The wedge method shown in B and C is good, an objection to B being that it is rather difficult to drive the wooden wedges in from the ends, whereas in C the slot has a groove on either side for the wood to rest in, and it is therefore much more easily fitted. There is, however, the objection that the wood is not in the best position to resist pressure from centrifugal action: very cross-grained wood should be used. A parallel slot has the advantage

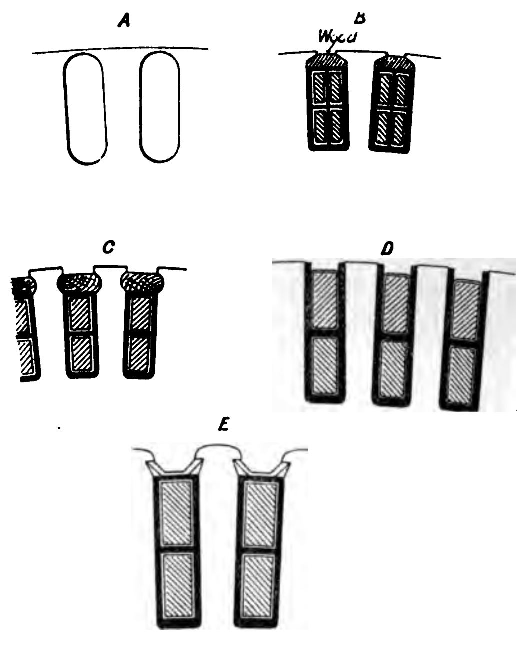


FIG. 13.

that it can be cleaned out by a milling cutter, and the winder may see much better what he is doing. The writer suggests using short buckled strips of German silver or other suitable material, as shown at E.

The simplest way to set out the width of slot and tooth is to make them about the same width at the periphery, so that the slot being parallel the tooth is rather smaller at the root. In order to guard against the teeth breaking off in handling, etc., the two corners at the bottom should be rounded out, and it is also an advantage to round off the

outer end of the tooth, as shown at E, Fig. 13, as by that means eddy currents on the pole face are reduced.

A somewhat novel slot (see Fig. 14) is used in the Bergman machines. The curvature admits of the coils being placed in position more easily, whilst the air space along the centre of the tooth increases the reluctance of the cross magnetic path.

The tendency of the future will no doubt be to follow traction motor design, and the number of slots for a given diameter of core will be much reduced. In the G.E. 800 motor for example, there were 105 slots with 6 wires in each, whereas in the more modern motor, G.E. 52 (which by the way has laminated poles) there are 29 slots with 24 wires in

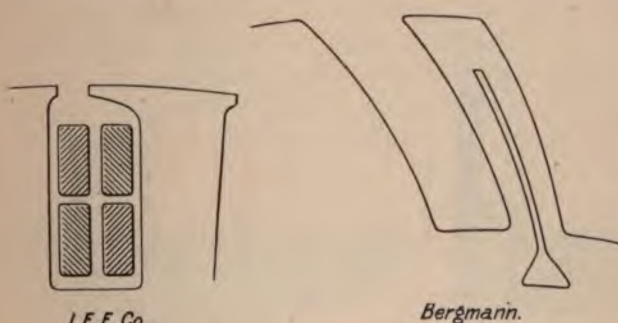


FIG. 14.

each. Ganz & Co. exhibited a machine at Paris which, by having a little care taken with the shape of the laminated pole-piece, gave good results with an unusually small number of slots for the size of the machine.[1]

WINDING OF ARMATURE.

The barrel or straight-out drum winding is now almost universal, and it may be interesting to note that it was first introduced into practical work by Parsons, on the original

[1] In the *Electrotechnische Zeitschrift*, for November 15, 1900, G. Dettmar, of Hanover, has an interesting article, in which he shows by actual experiments with iron filings, the disposition of the lines of force in the air-gap of dynamos and alternators. He gives the interesting rule that the distribution of lines takes the form of an isosceles triangle, of which the height is double the base, and from this it would appear that the partially enclosed slot, as in Figs. 13 B and 14, is the best arrangement. Clearly the effective armatur surface for field magnetism should be as large as possible.

turbine sets made by Clarke, Chapman & Co. Ordinary stranded cable was used, laid in round-bottomed slots milled in the core, the cable being bent into shape at the end as it was being wound on. As the machines were two-pole, the space taken up at the ends was, of course, rather excessive. When applied to multipolar dynamos, however, the barrel winding shows to great advantage, as the larger the diameter, and the greater the number of poles, the less in proportion is the space taken up by the diagonal end connections.

When the conductor is small in section the coils may be made in one piece, the sharp bend at the corner remote

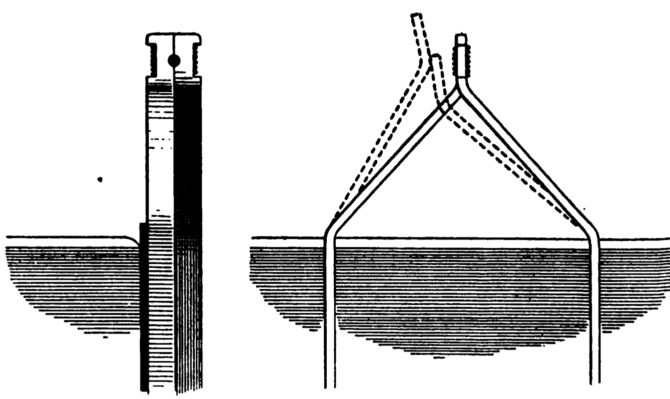


FIG. 15.

from the commutator being turned on a former, or if the conductor consists of thin strips it may be negotiated by folding the strip. Heavy section bars, where there is only one turn per segment, are generally arranged in halves. The first layer is placed in the slots with the projecting ends turned at an angle varying with the number of poles, the second layer is then placed in position, and the free ends are sweated together. More joints are thus introduced, but it makes a neat job, as it is possible to remove one of the upper bars without disturbing any other coil, whilst a lower bar can be removed without disturbing more than one half as many coils as would be necessary with the complete coil made in one piece. This is an important matter, because removing coils after they have been running some time is

very likely to crack their insulation, and cause further trouble. With series armature winding half coils are particularly convenient.

It may be mentioned that the joints will give trouble if not carefully made, as a very slight movement circumferentially causes one bar to ride over the other, as shown dotted in Fig. 15. To prevent this the writer has suggested keying the conductors together by drilling a hole in the joint, and filling with a plug of solder or harder material.

So much harm is done to insulation by the continual tapping with mallets that the writer thinks it would be well worth while to bend the conductors by means of levers shod with rollers, in much the same way as is used in shaping Thorneycroft boiler tubes. Any one who has seen the accurate and quick way in which steel tubes are bent when cold will recognise the great advantage there would be in applying the system to bending armature conductors. In Fig. 16 the writer has sketched a suitable arrangement.

It does not appear to have occurred to any one that it is quite unnecessary to bend both ends of each conductor. Clearly it would be much cheaper and more satisfactory from an insulation point of view to bend the conductor at one end only as shown in Fig. 17. In this way it could be slipped through the micanite tube from one end of the tunnel, and the present unsatisfactory wedges, piano wire bindings, etc., done away with altogether. As the micanite tube could fit tight in the slot no wedge would be required, and it will also be noticed that the micanite tube can in this case be carried right along to the end of the straight bar, only the bent portion being taped. The writer further suggests that one end of the conductors be held against centrifugal force by the end ring F, as in Fig. 9, and the other end by the upright of the commutator segment being bent over and riveted.

Internal Circulating Currents.

Exception has often been taken to the parallel-wound armature, because unless the armature is in true magnetic balance some of the circuits may generate current at a higher pressure than others, causing internal circulating currents. Unequal air-gaps or poles will also cause this.

Connecting the field coils in two parallels, those above

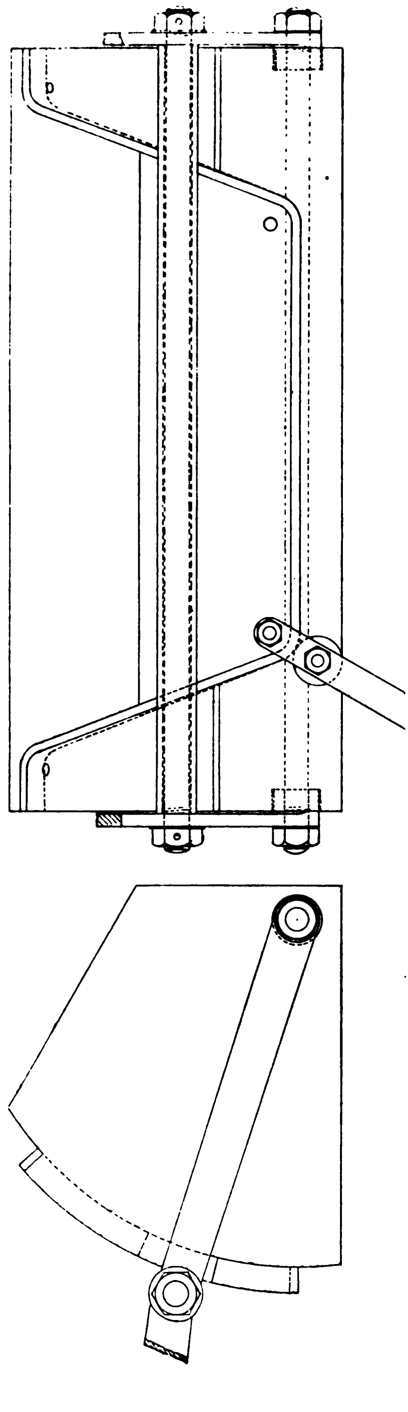


FIG. 16.—Design of Bending Block for Armature Conductors.

the spindle in series and those below the spindle in another series, enables a certain amount of adjustment to be made to counteract wear in the bearings, but great care must be taken or else unbalancing will result from the conductivities of the two halves not being equal at all temperatures.

By building the dynamo with connections as for a rotary converter, that is to say, connecting equipotential points, it is said that the unbalancing and uneven pull largely disappears. See B. G. Lammes' English patent No. 28736 of 1896.[1]

The Westinghouse Company have employed the method, but their more recent machines are provided with special conductors at the bottom of some of the slots, one end of

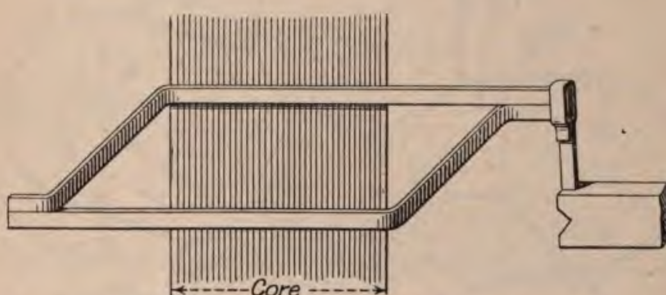


FIG. 17.—Conductors bent at one end only to pass through Tunnels.

such conductor being connected through to the commutator and the other to a short-circuiting ring, the number of such rings varying with the number of poles. These special conductors are also said to prevent surging of the magnetism in the field circuit.

The winding most generally adopted by Continental makers is that known as the Arnold series parallel winding. (See Fig. 18.) In the ordinary parallel winding it will be remembered that the numbers of slots and commutator segments must be even, whereas in the series winding the

[1] The argument is that by being cross-connected at two or three points of equal potential per pole, the winding under each pole is thus practically as in a synchronous alternating-current motor, and the cross-connections allow magnetising currents to flow from pole to pole to mutually adjust the field strength just as occurs when synchronous alternate-current machines are connected in parallel. In order to be able to do this, the number of commutator segments for a multiple circuit winding must be a multiple of the number of poles.

numbers must be odd. In the Arnold series parallel winding the slots and segments are even, the winding being a re-entrant series winding with an even number of slots and segments. In commencing at, say, a positive brush and going completely round the armature, the conductor does not arrive at the segment next to the one started from as in an ordinary series winding, but finishes at a segment

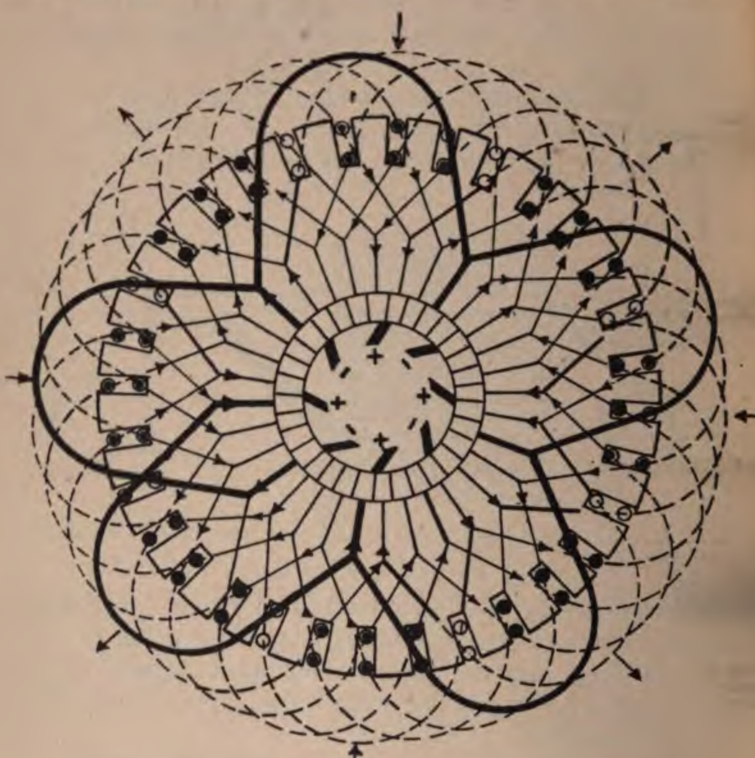


FIG. 18.—The Arnold Series Parallel Armature Winding.

somewhere near to the next negative brush. The only objection to the winding is that certain ratios must be observed between the number of poles and the number of slots; on the other hand it has the great advantage that, although a series winding, there may be as many brushes as poles, or only two sets of brushes, as in plain series winding.[1]

[1] Most of the Continental machines given in Table I. below have the Arnold winding.

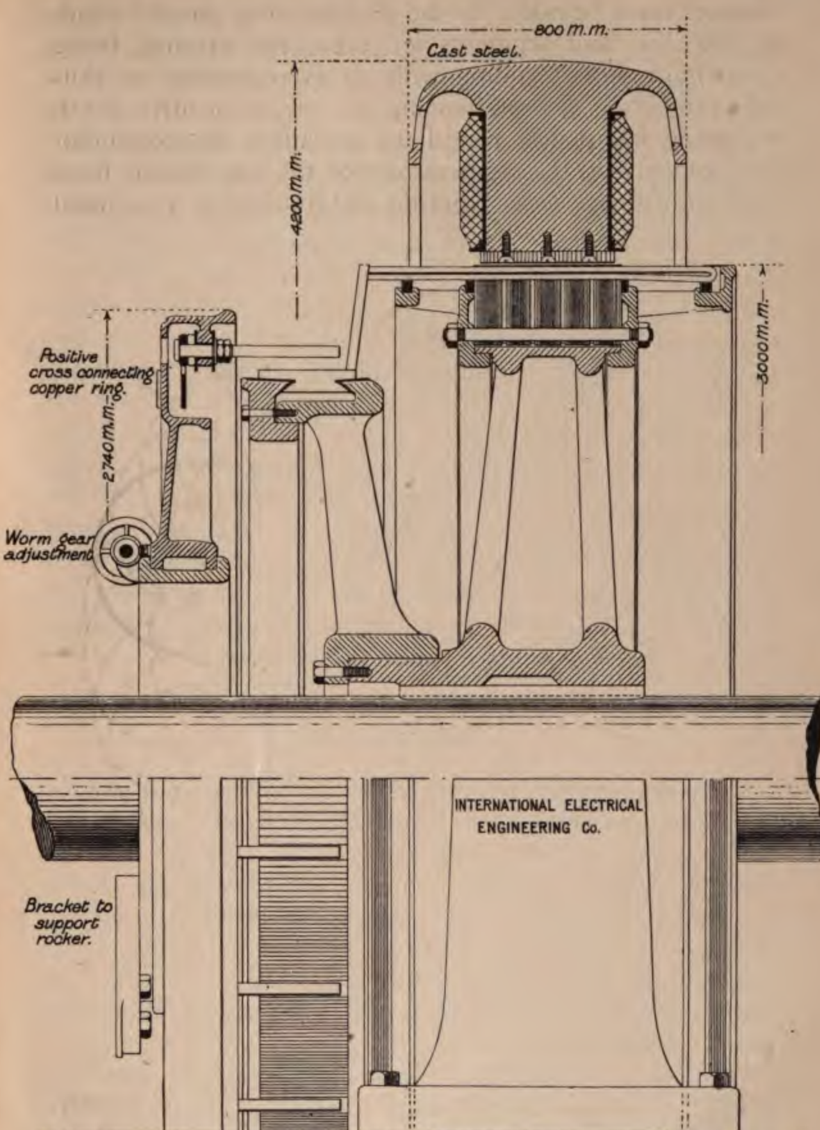


FIG. 19.—Slow-Speed Dynamo, 1,000 k.w., 85 revolutions.

Another method of equalizing the magnetic circuit is for the pole shoes to be bolted up to iron rings, all Norths to one and Souths to another. The shoes fit close to the poles, and the section of each ring bears a definite ratio

to the rest of the magnetic circuit. The brush ring is mounted on one of the pole rings, and there is an arrangement whereby the pole shoes can be adjusted in a rotary direction.

It is now a common practice in large output parallel-wound machines to keep the conductors down to a reasonable size by having two, three, four, or more circuits. This gives two or more commutator segments all at the same voltage, and the brushes span many segments instead of only covering a little more than one, as in single-wound armatures. The length of the commutator is thus decreased for a given current output. Multiplex armature winding, as it is called, practically changes an unwieldy conductor into several smaller conductors, and the eddy current loss is reduced. As the commutator bars of one winding are not adjacent to each other, but alternate with the bars of the other windings, there is not so much risk of a section being short-circuited. Multiplex winding is especially useful where a dynamo is built for 230 volts and requires altering afterwards to 460 volts, whilst for very large outputs the method becomes a necessity on account of commutation.

COMMUTATORS.

If commutators are not provided with a solid support on the inner side of the segments a blow on the surface may produce a flat. The trouble arises partly from the difficulty there is in getting metal segments and mica strips exactly to gauge, and also because when the parts are clamped together and the inner surface bored out it is hit or miss whether the segments fit tightly on to the insulation. The skill of the workmen enters largely into successful commutator construction.

Undoubtedly the best way of holding commutator segments is by vee-shaped micanite rings, which are now readily obtainable in standard sizes. For the mica between segments, continuous strips are better than built-up mica, for one reason because in building up the latter there is danger of getting conducting particles in between the laminations. Of the many qualities, only soft amber mica is suitable for strips, which are cut from the solid, as at the usual thickness of ·035 of an inch, experience shows that

amber mica wears down at about the same rate as hard-rolled or drop-forged copper. Built-up mica wears down more quickly, and therefore the many varieties of harder mica may be used in its construction.

Much trouble has been caused by the commutator lugs snapping off short at the commutator bar, and in order to insure against this it is now usual to key the commutator as a whole on to a projection of the armature hub so as to

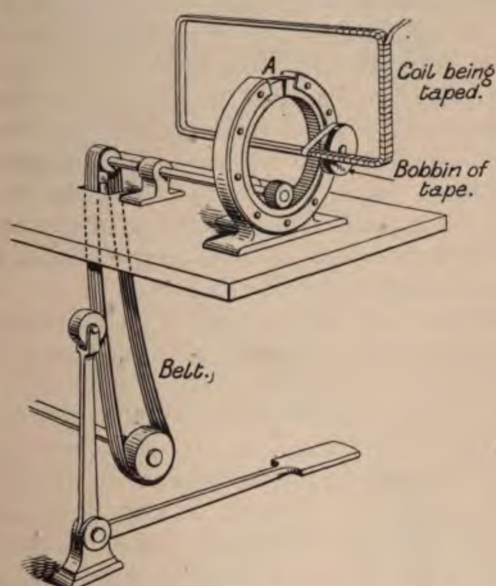


FIG. 20.—Machine for Taping Closed Coils.

prevent them moving relatively, expansion of the lug being also provided for by slightly buckling it midway in its length. In some cases where dynamos have been driven by high-speed steam engines, the breaking of commutator lugs has been traced to a periodical vibration set up by the cranks, the armature, and the barring flywheel; in fact, it was this which led Messrs. Willans and Robinson to insist on the barring wheel of their engine being bolted up direct to the armature hub, as shown in Figs. 2 and 21.

A feature of many foreign machines is that the commutator lugs are made of high resistance metal, such as German

silver, Rheotan, etc., the idea being to assist sparkless reversal. Other makers use quite small section flexible connections from the armature conductors down to the commutator segments, so introducing resistance, and also ensuring that the connections shall not be broken by vibration.

It is also becoming quite common, on the Continent especially, to fasten the armature connection to the commutator segment by means of steel grub screws and so dispense with solder. A conductor, quarter-inch diameter, will have as many as three grub screws, each half-inch in diameter, to hold it in position.

In some machines special efforts are made to keep the commutator cool. For example, the dynamos in the Luisenstrasse Station in Berlin are fitted with a small electrically driven air blast, which also removes any copper and mica dust from the armature, etc. Some of the Schuckert and Lahmeyer dynamos have slots cut right through the segments to allow air to circulate through.

Commutators for large output machines which are coupled to high-speed engines must of necessity run at very high peripheral speeds. In some such cases sparking difficulties have been experienced, but they do not, however, appear to be due to the fact that at high speeds good commutation is impossible, but to deficient mechanical design. There is no reason why dynamos of 1,000 k.w. and upwards should not be built for coupling to Belliss, Willans, or other similar engines if the commutator be built with its hub of cast steel and the commutator sections of high drawn copper made of buckled section so that the adjacent segments support each other. Such a commutator pressed up in a hydraulic wheel tyre press will be equal to anything reasonable in the way of high peripheral speed.

Brush Gear.

The question of the design of carbon brush holders presents quite a little problem in itself, but as it has been dealt with at considerable length elsewhere (see Parshall and Hobart's "Electric Generators," Fischer-Hinnen's "Continuous Current Dynamos," the writer's articles in the *Electrical Review*, April 22 and May 6, 1898, etc.), it will

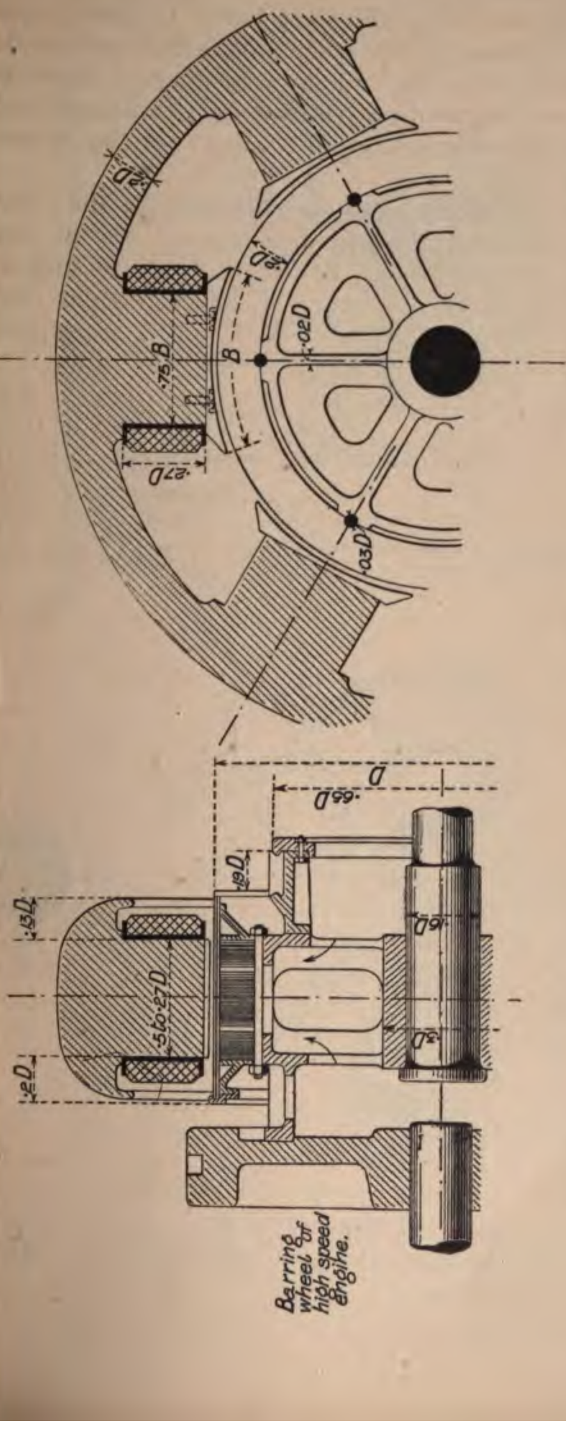


FIG. 21.—Approximate Proportions of a Multipolar Dynamo suitable for Coupling to a High-speed Engine.

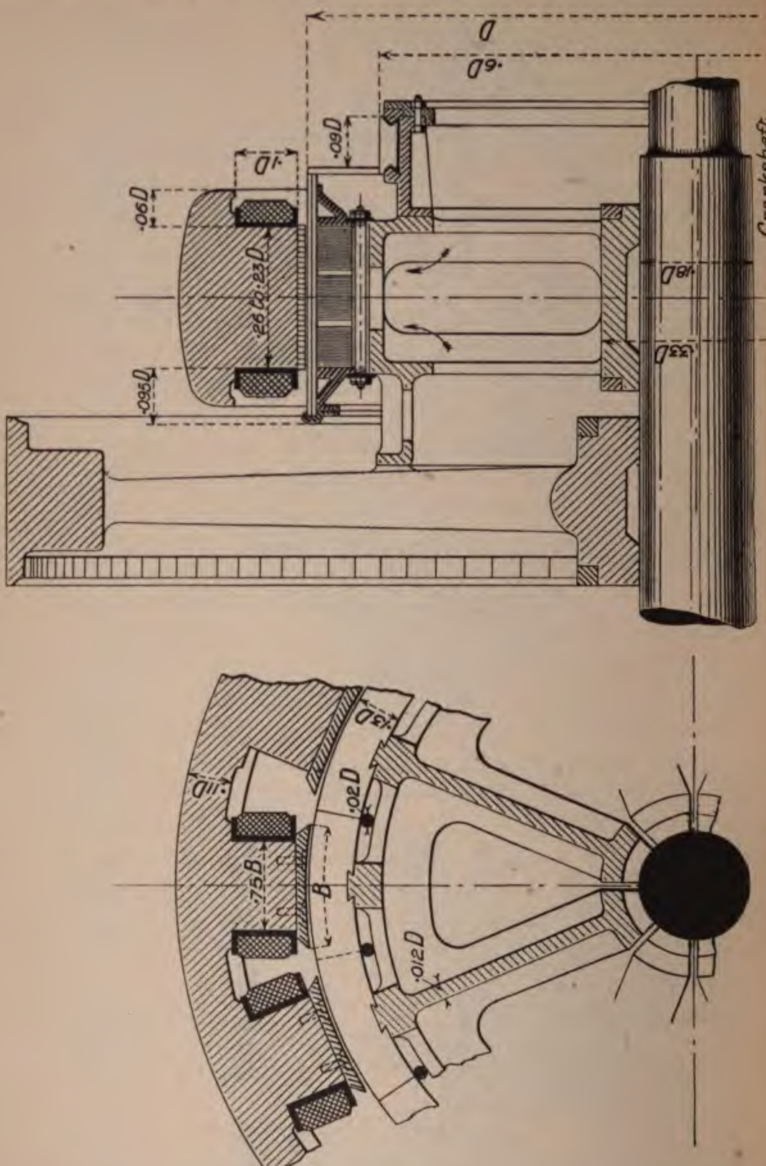


Fig. 33.—Approximate Proportions of a Flywheel Dynamo.

be only necessary to summarise here some of the points which the writer's experience has shown to be essential to a good brush holder :—

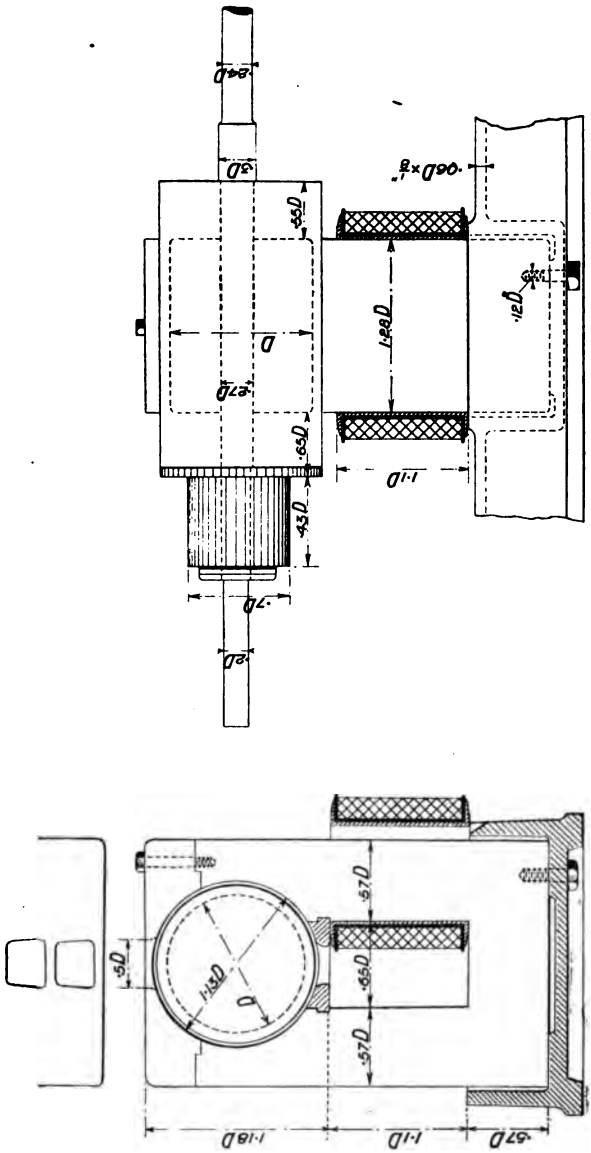


FIG. 23.—Approximate Proportions of a Bipolar Dynamo.

(*a*) The carbon block *firmly fixed* at the end of a fairly long arm.

(*b*) The inertia of the moving parts reduced as much

as possible. This may be effected by constructing the holder of aluminium or sheet metal.

(c) Current taken from the carbons by short pieces of flexible soldered direct into the blocks.

Some makers arrange the carbon blocks exactly square to a whole number of segments, and many prefer loose carbons pressed down by a spring finger. These do very well for traction motor work, but for dynamos the number of separate blocks becomes considerable, and the chatter and noise of loose carbons is too great, even when they are of the ingenious reaction type. There is always a considerable drop of volts with carbon brushes, and where efficiency is a prime consideration, as in a dynamo, the current must be carried as direct as possible to the main terminals by flexibles soldered direct into the carbons.

A favourite method of carrying the circular brush rocker is by means of four or more arms bolted to the yoke, but the writer does not like this method for the following reasons: If the arms are brought to the outer end of the commutator they cover up the brushes at various points, whilst if the arms are brought down to the middle or to the inner end of the commutator much space is uselessly occupied. Again, in case the yoke has to be racked sideways or the top half removed, then the whole of the brush gear must be unshipped. Where it can be conveniently arranged the writer considers that the brush rocker should be carried from a bracket bolted to the bearing casting, as shown in Figs. 2 and 19.

Brush rocker rings and the attachments of the spindles, etc., are often too weak in design. Being the only part of a dynamo which the station attendants have to handle, everything about it should be massive, and yet at the same time the fitting and design such that the brush adjusting gear can be worked without appreciable effort.

Fig. 19 indicates this massive construction, and a neat feature is the boxing in of the ends of the brush holder spindles and connections, as well as the two heavy copper rings connecting the positive and negative sets of brushes respectively.

A feature which many dynamos lack, is an arrangement *by which* the attendant may raise and depress the brushes

all together. At a certain station, for example, there is an 1800-kilowatt dynamo with 20 poles and 9 separate brush holders to each, which makes 180 brushes in all—many of them in an inconvenient position. To ask the dynamo attendant to lift all these brushes separately is rather too much, and makers would no doubt find it well worth while to add the small extra piece of gearing required. It is easily arranged by means of bell crank levers at the ends of the brush spindles.

ELECTRICAL INSULATION.

For insulating armature slots some makers use layers of press-spahn (a red coloured pure wood fibre paper finished

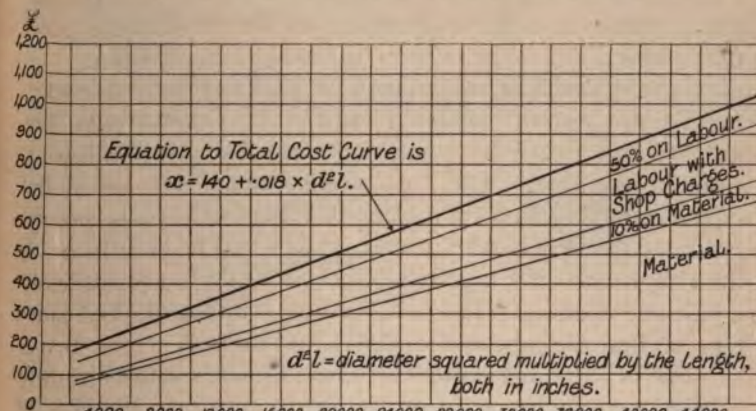


FIG. 24.—Cost of Dynamos suitable for Direct-coupling to Engine. No Bed-plates.

with an oily glaze), whilst others employ micanite troughs or tubes made to the exact size required. Many manufacturers use press-spahn, micanite, and oiled linen, and one firm mills out the slots full large and then lines them with strips of teak wood. Press-spahn is a very superior insulation if it is kept dry, the writer having flashed Siemens armatures at 7,000 volts which had the slots insulated with this material alone. It is risky to depend on it altogether, however; mica in some form or another should be employed. Mica has one advantage over all other forms of insulation in that it has exceedingly high disruptive strength, and is not affected by atmospheric *conditions*. Great care must, however, be

taken in selecting and making up the mica. The writer is of opinion that the best results can only be secured by the dynamo manufacturers making their own micanite and other insulation.

As micanite channels or tubes only protect the conductors where they pass through the core, it is necessary to use cotton covering or tape, etc., elsewhere. For large conductors taping is essential, and it is usual to tape over double cotton covering. It is good practice to use plain cotton tape and serve with varnish after it has been wound on, because the various adhesive tapes are so awkward to handle. They also lack mechanical strength and generally contain a fair amount of moisture. The shellac should be made with best alcohol; wood alcohol is of no use. The baking must be done thoroughly, or the alcohol will attack the copper.

Taping formed coils or bent conductors by hand is an exceedingly tedious process. It can be done much more quickly and neatly by machinery, and Fig. 20 illustrates a suitable machine. The coil is introduced into the centre of the revolving ring by means of a diagonal notch, and the bobbin of tape is carried on a pin attached to this ring. As the ring rotates the tape is wound on to the conductor, and a pair of rollers grip the coil, feeding it along at the proper rate, so that the tape is accurately half-lapped.

One of the most important questions in connection with insulation is—what kind of varnish to employ. Shellac has been most used, but the quality varies so much that other special varnishes and paints, etc., are now employed.

The Monarch Asphalt Paint and the P and B Compound are used in America, whilst a fair number of makers are adopting pure linseed oil. Although it has not very great disruptive strength, or high insulating resistance, it is found that when oxidised at the proper temperature to expel moisture, linseed oil is very reliable. It is practically non-absorptive and is only affected by a temperature far higher than that which destroys the cotton insulation into which it is soaked. Cotton which has been shellac-varnished becomes brittle, whereas when oil is used it remains flexible. Only pure linseed oil should be used, and care must be taken to lay it on very evenly. Of course, for winding magnet coils the double cotton-covered wire may be run through hot

paraffin immediately before it is wound on ; or the writer suggests placing a number of coils in a vacuum oven, and as soon as all moisture is driven off, flushing them with boiled resin oil in the same way that cables are treated.

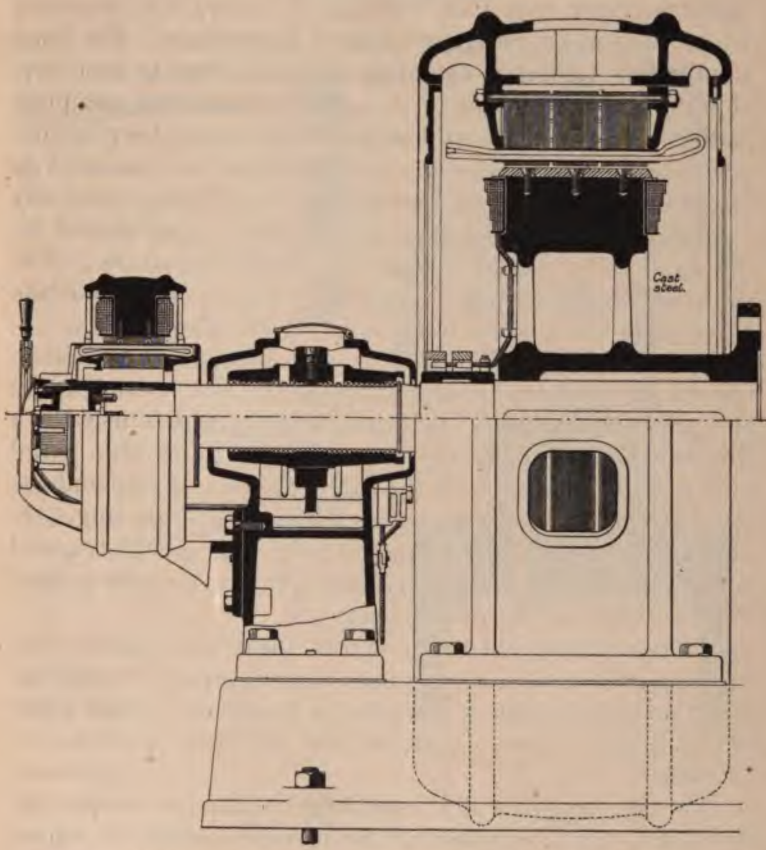


FIG. 25. Multiphase Alternator (I.E.E. Co.) for Coupling to High-speed Engine, as supplied to Erith U.D.C.

An easy way to varnish an armature is to pour the varnish, armalac, or whatever is used, into a shallow bath, and support the armature in journals, so that its periphery just dips into the liquid. By revolving slowly, brushes can be dispensed with altogether.

Some day, possibly, a cheap compound will be discovered which will not soften or get brittle when the

armature warms up, and into which the armature can be dipped overhead in much the same way that a transformer is dipped in ozokerite or diatrine.

FIELD WINDING.

In very large overcompounded machines, such as are used in traction work, the sectional area of the series winding is considerable, and it is often a somewhat difficult matter to arrange it conveniently. One method employs ordinary stranded cable—61/11, 91/11, and so on—according to the current to be carried, the coils being supported from the magnet yoke so that they are quite independent of the shunt coils. Where the section of the pole is circular or oval, bare copper strip of slightly taper section may be wound on edge in the manner introduced by Mr. Ferranti, the insulation being by discs of rice paper or presspahn. An objection to this method is that the copper cannot be unwound and used again, as is the case with ordinary flat tape. Another method applicable to rectangular poles is to wind on a flat copper ribbon of the full area required, and rather less than half the width between the flanges of the field magnet former. A broad sheet of longcloth and mica insulation a little wider than the copper is wound on with it, and there are thus two turns per layer.

In order to avoid bringing out the inside end of the field magnet coil, two sections are wound in opposite directions, so that when placed side by side the inside ends are connected together, and the free outside ends form the terminals.

To save expense and complication the compounding turns may very conveniently be carried round the north poles only.

There is not much to say regarding shunt windings, except that in large machines it frequently happens that a standard gauge of wire does not give the correct ampere turns. The usual practice in such cases is to employ the next two sizes of wire, large and small, and adjust the turns of each until the required current is obtained. As it is exceedingly important that each pole must have the *same magnetic strength*, it is necessary to be careful to get just the *right* quantity of each size of wire on each pole. On this

account, and also because of the joint required, it is good practice to take the next larger wire and draw down to the exact diameter necessary. The equipment of an up-to-date dynamo works is certainly not complete without wiredrawing, cotton covering, braiding, and taping machines.

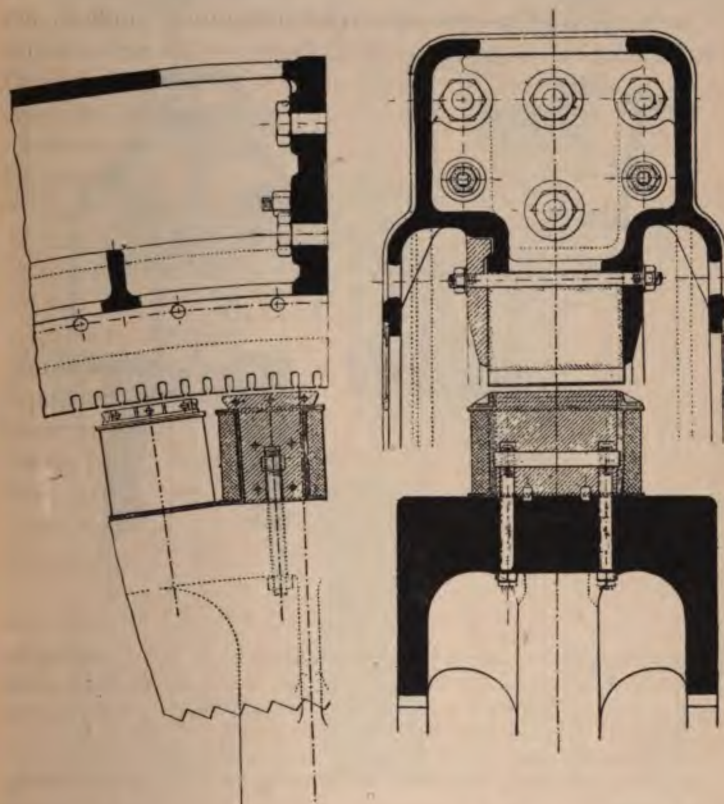


FIG. 26.—Three-phase Alternator by Compagnie de Fives-Lille. Output 800 k.w., 2,200 volts, 50 cycles, 79 revolutions, 76 poles. Diameter over Poles 235 inches, giving Peripheral Speed of 4,850 feet per minute. Air-gap 0·275 inches. Diameter of Shaft 25 inch.

The energy stored in the magnetic circuits of large dynamos is so very considerable that it is always as well to make a short-circuiting shunt break switch part of the machine, so as to ensure that it shall be always used. At the same time the insulation of the coils should be flashed with a *pressure at least* five times the normal voltage.

Magnets may be surrounded by a sheet of brass, copper, or zinc, forming part of the spool, as it is noticed that such an arrangement reduces the flash on breaking circuit. There is also an old dodge in telegraphic instrument making, which has for its object the reduction of the self-induction spark. It is to wind the layers *always in the same* direction, instead of backwards and forwards.

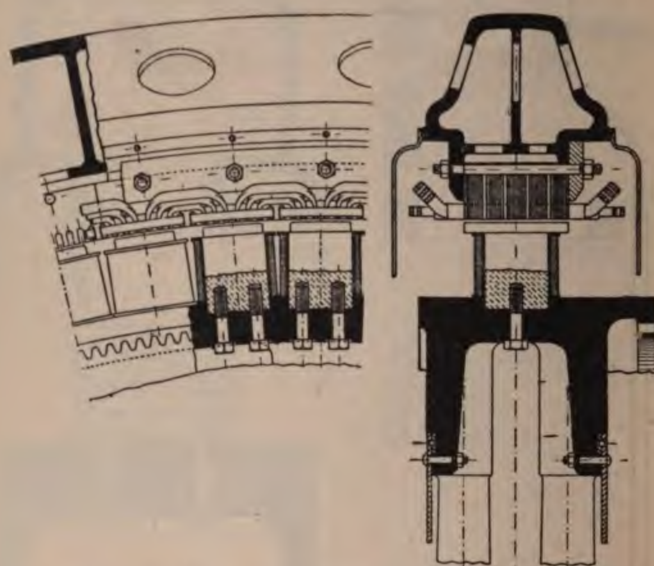


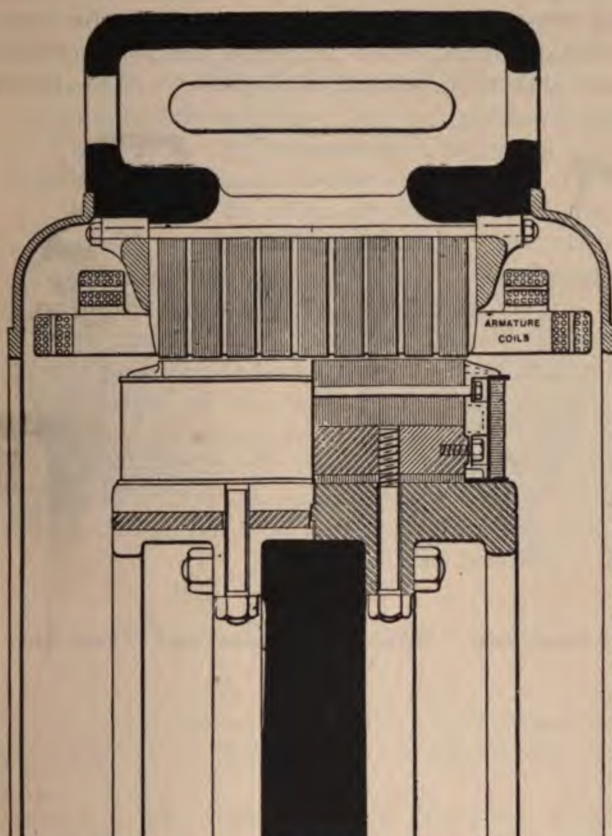
FIG. 27.—Three-phase and Single-phase Alternator by the Helios Elektricitäts A.G. Output as Three-phase 3,000 k.w., and as Single-phase 2,000 k.w., or if giving both together the Outputs are 1.500 k.w. and 1,200 k.w. respectively. Voltages 6·600/3,300/2,200, 50 cycles, 71·5 revolutions, 84 poles. Diameter over Poles 315 inches, giving Peripheral Speed of 5,900 feet per minute. Air-gap 0·47 inch. Diameter of Shaft 23·7 inches.

This has also the advantage that the thread is always the same, and so the layers pack better.

BAKING.

Baking is one of the most important operations. If an oven is employed it should be very spacious and well-lighted, with several small iron doors fitted into the large doors. There should be trolley lines, so that the heavier armatures, etc., can be run in and left on the trolley, for

when anything heavy has to be handled in a hot room it is apt to be knocked about. The temperature should be about 170° F., but for anything special 200° F. to 300° F.



IG. 28.—Three-phase Alternator by G.E. Co. of America for the Metropolitan Street Railway Company of New York. Output 3,500 k.w. (5,000 k.w. for 4 hours), 6·600 volts, 25 cycles, 75 revolutions, 40 poles. Diameter over Poles 200 inches, giving Peripheral Speed of 3,900 feet per minute. Air-gap 0·312 inch. Diameter of Shaft (hollow) 37 inches.

useful. Foundry core ovens are sometimes used for ese higher temperatures.

In certain cases, where the oven is full or the apparatus be dried is unwieldy, moisture can be driven off by ssing through it a large current at a low voltage, surunding the coils with asbestos board to localise the

heat. In this case, as the heating goes right through the coils, it is more effective than oven drying, but the electrical method is of course more expensive.

Undoubtedly a very effective method is to use a vacuum chamber. The Passburg apparatus is the best known, and it consists of a circular shell five or six feet diameter, with air-tight door and glands. A steam coil is arranged inside, space being left for one or more sets of rails to support the workshop trolley. The outside of the chamber is lagged with non-conducting composition, and the steam in the coil is at about 75 lbs. pressure.

The accessory apparatus consists of an air pump giving a vacuum of 20 in. to 29 in., a small surface condenser to take up any moisture drawn off, and also a testing apparatus to indicate when all the moisture has come away. This apparatus has been adopted by all the leading Continental firms, some firms having as many as ten or twelve in their works. An apparatus five feet diameter, with pump and condenser complete, costs about £260, but there are many advantages to be derived from its use, not the least of which is that an armature or coil may be dried in a fraction of the time that it would take in an oven.

Tables, etc.—Continuous-Current Dynamos.

A very considerable amount of data of actual machines is now available, and with a view to crystallising as it were many of the particulars, the writer has compiled Tables I. to VIII.

Table I. gives data of various multipolar machines, and Table II. coefficients deduced from the particulars in Table I., the figures being given in English measures. Table III. gives coefficients deduced from data given in Professor Arnold's book on Armature Windings.

In settling the preliminary dimensions of any dynamo the two leading dimensions are the overall diameter of the armature in inches d and the overall length of the armature core in inches l. From these we can obtain various useful figures. The first, which may be called the "*size constant*," is obtained by multiplying the diameter squared by the length of the core

$$(d^2l).$$

In the second (due to Steinmetz) the diameter, multiplied by the length and divided by kilowatts, gives a coefficient $\left(\frac{d \times l}{\text{K.W.}} = \text{coefficient}\right)$ which varies according to the size of machine. See the 9th column in Table II. and the 10th column in Table III. It may be taken as ranging from 2 for the largest flywheel generators up to 8 for the smaller sizes of multipolar machines, but as will be seen from Table II., a dynamo by Siemens Bros. and Co. gives the unusually low figure of 1·4.

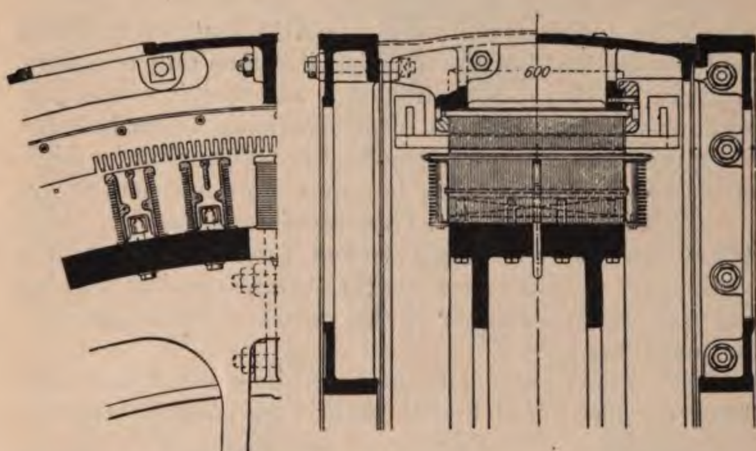


FIG. 29.—Three-phase Alternator by Siemens and Halske. Output 2,000 k.w., 2,200 volts, 50 cycles, 83·5 revolutions, 72 poles. Diameter over Poles 235 inches, giving Peripheral Speed of 5,150 feet per minute. Air-gap 0·47 inch. Diameter of Shaft 19·8 inches.

The objection to the Steinmetz formula is that it can only be used within limits, as it takes no account of speed. For this reason the better known "*output equation*" below is to be preferred. In this the output in watts is equal to a coefficient multiplied by the number of revolutions per minute, by the diameter squared, and by the length in inches.

$$(\text{Watts} = \text{coefficient} \times n \times d^2 \times l.)$$

The figure in this case is a decimal quantity, and from data taken of many machines the writer finds that it varies somewhat as follows :—

	Coefficient for output equation.
For the largest flywheel generators	·033
„ large multipolar dynamos	·03
„ medium size multipolar dynamos, say about 300 K.W. ...	·025
„ small multipolar dynamos	·018

To continue for bipolar dynamos the values are :—

For large bipolar dynamos	·014
„ medium size bipolar dynamos, say about 30 K.W. ...	·01
„ small bipolar dynamos	·007

With the help of this formula, given the output of a machine and its speed, we can find d^2l, and having found the "*size constant*" of the machine, it is then only necessary to divide the two factors in such a way that it meets proportions which are known from experience to be best for any given carcase. For example, it is known that for high values of d^2l the armature becomes more and more "*fly-wheelly*," to use one of Dr. Thompson's expressions. With this in view the writer has prepared Table IV., giving proportions for various values of d^2l; also the number of poles and approximate weights of the various essential parts of the machines. [1]

With regard to the detailed dimensions of the machines, the writer has found that these may be roughly taken as a percentage of the diameter as indicated in Figs. 21, 22, and 23, the results given in the figures being averaged from a number of examples. Although at first sight the method may seem crude, the writer has found a wonderful agreement between many varying designs. In the case of bipolar machines the whole of the dimensions may be given as a percentage of the diameter of armature, but in the case of multipolar dynamos the pole dimension measured circumferentially is arrived at by multiplying the number of poles by four and then allowing three of the parts for a pole and one part for the space between the pole tips (see Figs. 21 and 22). Calling the dimension measured across the face of the pole between the pole tips B, the dimension across the pole inside the magnet coil should be ·75 B. Of course these percentages are only intended to be approximate.

[1] *As a comparison* Table V. has been prepared on the same lines for bipolar *machines.*

Where the result is a decimal, then the nearest even figure should be taken. The final dimensions would, of course,

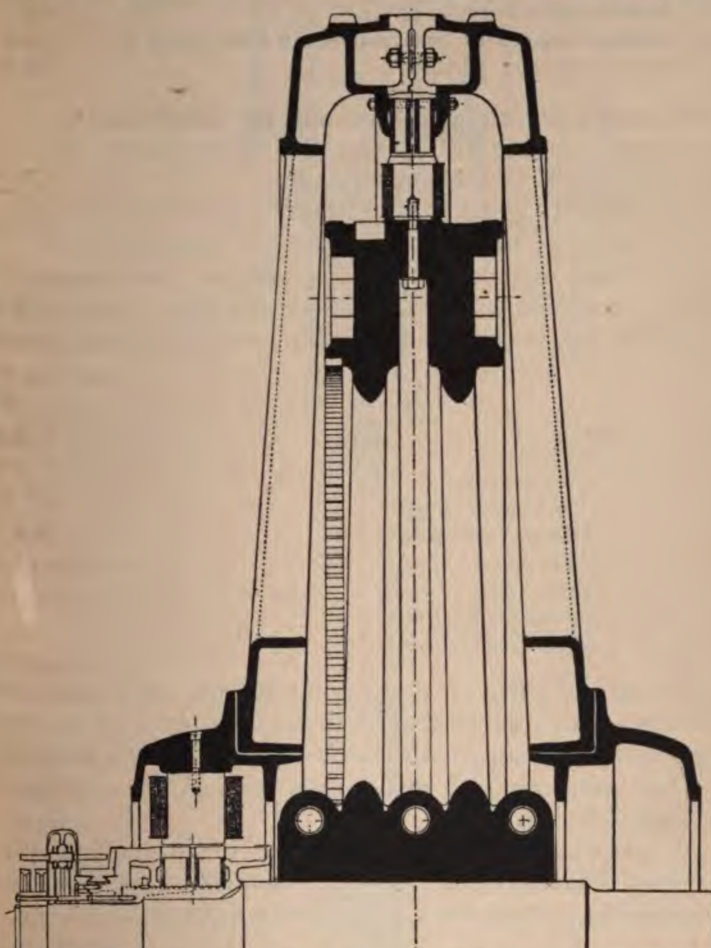


FIG. 30.—Three-phase Alternator by the Compagnie Internationale d'Electricité, Liège (I.E.E. Co., London). Output 1,000 k.w., 2,200 volts, 50 cycles, 83 revolutions. Diameter 216 inches, giving a Peripheral Speed of 4,720 feet per minute. Air-gap 0·335 inch. Diameter of Shaft 23·7 inches.

be fixed after the electrical calculations have been checked over.

Fig. 24 gives curves of actual cost of material and labour for a series of *multipolar* machines, and it is interesting to

note that when plotted in this way, that is, with the horizontal line in values of d^2l, we get practically a straight line. The writer has found the equation to the "*total cost*" line for a certain series of English machines to be

$$x = 140 + \cdot 018 \times d^2l.$$

Of course the curves would vary with different makers, and also according to whether the establishment charges were arrived at in the way indicated.

An interesting study for any given set of conditions in a given shop is the analysis of the cost percentages of the various items which go towards material. For example, taking a machine with an armature say 5 feet in diameter, the writer has found the following percentages hold fairly well :—

Materials—	Percentage of total cost of material.
Armature copper	6 per cent.
Field copper	19 "
Commutator copper	6 "
Armature stampings	12 "
Field poles	7 "
Yoke	20 "
Insulating materials	8 "
Miscellaneous	22 "

Of course it must be understood that the writer does not put these particulars forward as at all general. It is quite impossible to compare various workshops in this way, but any one shop can plot such curves and employ such percentages with advantage.

A rule which gives a direct clue to the diameter is that the current density per inch of circumference, or the "*circumferential-current density*," should be equal to

$$\frac{\text{the number of conductors} \times \text{the current in one conductor}}{\pi \times \text{diameter of armature in inches}}$$

If the result given by the formula is above 700, it indicates a skimped design, except in the very largest machines; and on the other hand, if it is below 300, the dimensions of the machine may be taken as being too liberal. Results of various machines by this rule are given in the last column but one in Table III. For the ordinary run of multipolar machines about 500 is a good average.

ree-phase Westinghouse Alternator for the Manhattan Elevated
Output 5,000 k.w., 11,000 volts, 25 cycles, 75 revolutions,
Diameter over Pole-tips 384 inches.

Of course in comparing the outputs of machines a great deal depends on the peripheral speed, and this varies between exceedingly wide limits. For example, the Siemens dynamo at top of Table II. runs at 5,320 feet per minute, whereas the average speed of the machines given in Tables II. and III.

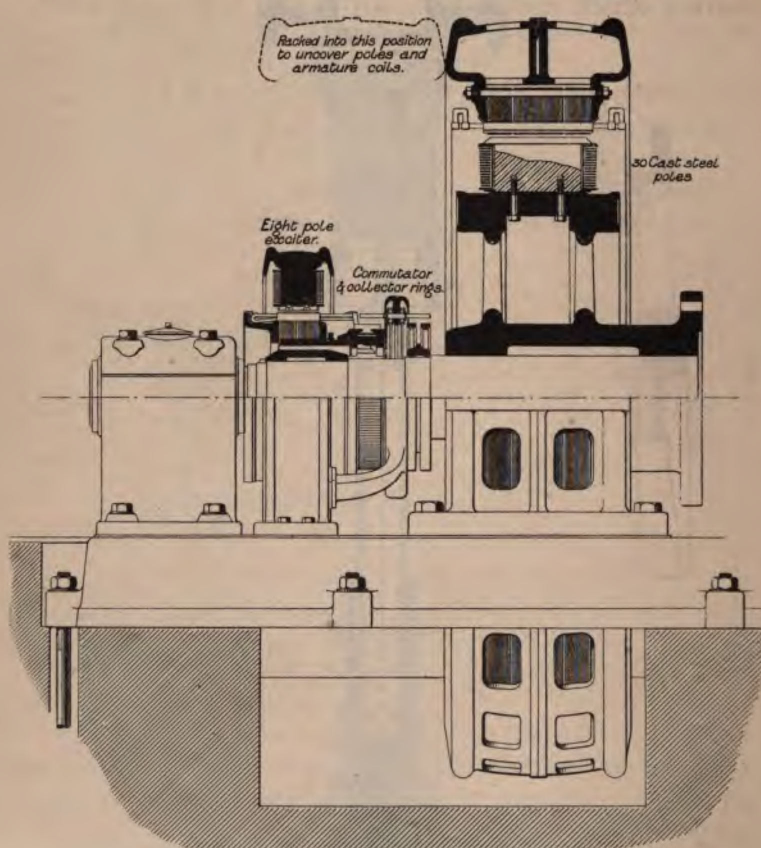


FIG. 32.—Design for a 1,400 k.w. Two-phase Alternator, 50 periods, to run at the high speed of 200 revolutions.

is under 3,000 feet per minute. Naturally the higher the peripheral speed the greater the output which can be obtained from a given sized carcase.

The Siemens machine above mentioned is curious in another respect, in that the peripheral speed of the commutator is as high rate as 3,420 feet per minute. There are

only three cases in Table II. and four in Table III. where the peripheral speed exceeds the limit which has come to be regarded as general practice, namely, 2,500 feet per minute. The question of resisting centrifugal action calls for an even lower velocity on the commutator.

The number of poles is really fixed by the amount of current which can be collected at any one pole, and this

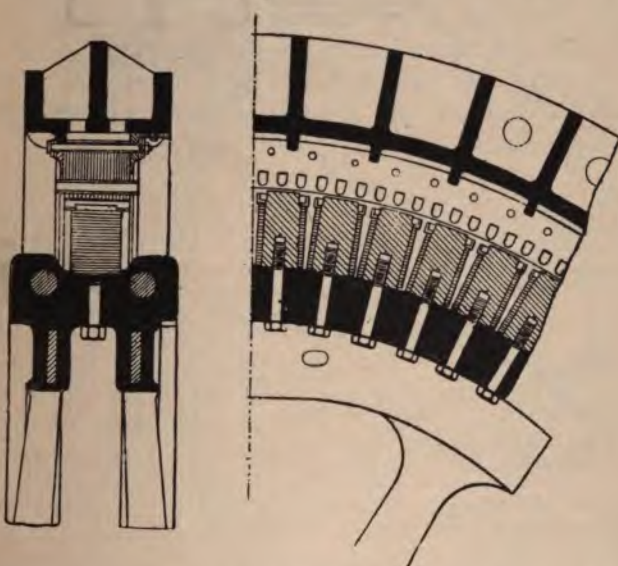


FIG. 33.—Alternator by A Grammont. Output 860 k.w., 2,400 volts, 50 cycles, 94 revolutions, 64 poles. Diameter over Poles 196 inches, giving Peripheral Speed 4,820 feet per minute. Air-gap 0·275 inch.

again is limited by the line density in the air-gap. An average figure may, however, be taken at 500 amperes per pole.

The standard practice in induction densities in the various parts of the magnetic current may be taken as follows :—

	Per sq. cm.	Per sq. inch.
Armature core	9,000 to 12,000 ...	58,000 to 78,000
" teeth	17,000 to 19,000 ...	110,000 to 123,000
Air-gap	7,000 to 9,000 ...	45,000 to 58,000
Cast-steel poles	13,000 to 15,000 ...	84,000 to 97,000
Cast-steel yoke	9,000 to 12,000 ...	58,000 to 78,000
Cast-iron *yoke*	5,000 to 6,000 ...	32,500 to 39,000

Designers appear to be practically agreed that to get sparkless commutation and a minimum zone of movement of the brushes, there must be a certain relationship between the ampere turns required for the air-gap at full load, and the cross ampere turns of the armature. Some say that the best relationship is when the one divided by the other gives unity or thereabouts. At the same time, many machines, especially those made in America for traction purposes, show figures which according to this rule would indicate a precarious design. Thus, if we take the four dynamos which are given in Messrs. Parshall and Hobart's book on Electric Generators, we get the following figures :—

Kilowatts	1,500	200	300	250
Amperes	2,500	400	2,400	455
Volts	550/600	500/550	110/125	500/550
Revs. per minute ...	75	135	100	320
Number of poles ...	12	6	10	6
Gap ampere turns at full load per pole...	6,000	4,800	4,900	4,150
Cross or distorting ampere turns at full load per pole ...	9,500	7,900	7,200	6,380
$\frac{\textit{Gap ampere turns}}{\textit{Cross ampere turns}}$	*·631*	*·61*	*·68*	*·65*
Current collected per pole	416	134	500	150

obvious that if good commutation can be obtained
-onstant

FIG. 32 Design

$$\frac{\text{Gap ampere turns}}{\text{Cross ampere turns}}$$

is und ower 00 t unity, then such a machine will be a periphe ie, bec eed e a large number of cross ampere turr obtaine hat a ver output is obtained from a ous in
The n
another r owing conditions tend to this result :— ie com-
mutator is a here are

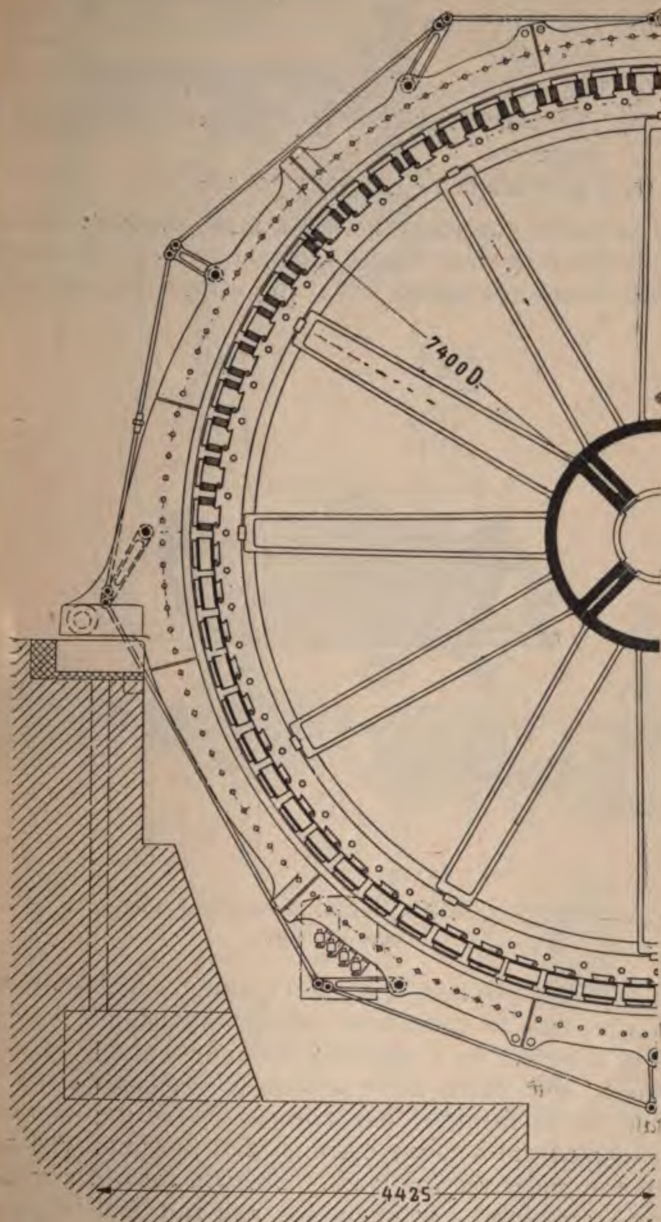


FIG. 34.—5,000 k.w A.E.G. Three-phase Alternator, showing Tie-rod Construction.

(*a*) The use of carbon brushes.
(*b*) The saturation of the pole shoe, especially at the pole tips.
(*c*) The saturation of the teeth of the armature core.
(*d*) The use of high-resistance commutator lugs for the segments.
(*e*) The fields being over-compounded.

The writer considers the last a most important feature because the *increase of magnetisation* occurring with increase of load comes into action just at the critical moment when

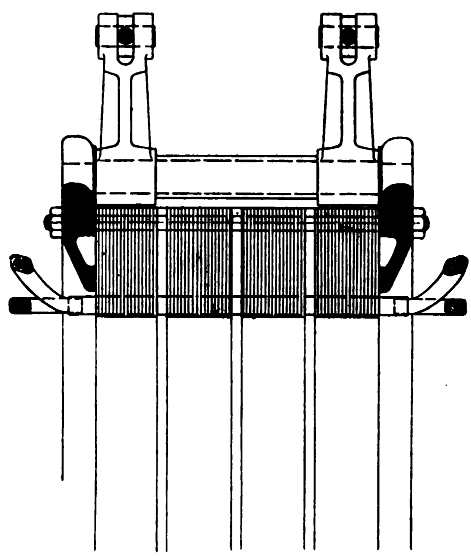


FIG. 35.—Detail of 5,000 k.w. A.E.G. Three-phase Alternator. Brackets for Tie-rods.

it is required. In fact, it is just a question whether it would not be as well to compound *all* dynamos, simply with this object in view. In a central station for lighting it would mean a little extra cable and switch apparatus, but this would be much more than covered by the greater output obtainable from a given machine and the improved commutation under varying loads. The dynamos in a traction station are always compounded and run well in parallel without trouble, so the writer cannot see why dynamos for *lighting* should not be compounded also. It is clear that as the motor load grows in any particular area the conditions

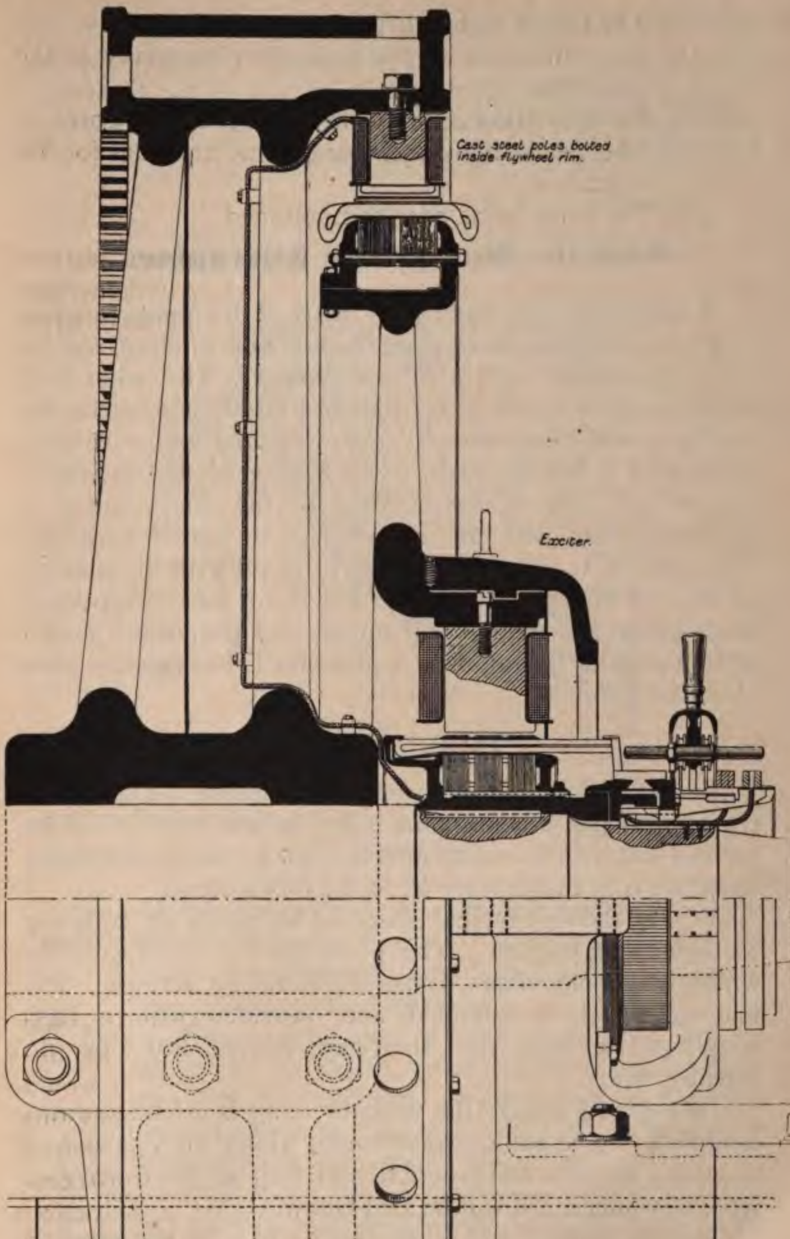


FIG. 6.—Outer-pole Type of Three-phase Alternator for supplying power at the British Xylonite Works.

which the dynamos have to meet approximate more and more to those of a traction generator. Of course the fact that practically all lighting networks are on the three-wire system, and that batteries are generally used are points in favour of keeping to shunt machines.

Part II.—Multiphase Alternators.

It will be evident that a good many of the points referred to above as to armature iron, methods of winding, insulation, etc., are also applicable to alternators. The writer is of course considering only the standard multiphase alternator having a stationary slotted core armature and revolving poles with a coil on each. The history of the alternator has been a case of the survival of the fittest, and it is significant that, with the addition of a slotted core, the alternator of to-day is practically identical with those made by the old Elwell Parker Co. For is not every iron-cored single-phase alternator to all intents and purposes a multiphase machine having only a portion of the armature slots utilised ?

Typical Designs.

In Figs. 25 to 36 the writer has collected a number of typical designs of multiphase alternators, and it will be noticed that with the exceptions of Figs. 33, 34, 35, and 36, the armature ring castings are all of the box section.

Fig. 25 shows a typical design of alternator for coupling to high-speed engines. The poles are cast solid with the wheel, the pole shoes being held on by screws. The magnet wheel is extended and provided with a half-coupling, so that the shaft-key carries little or no twisting stress.

In Figs. 29 and 30 the armature ring is extended down each side to give additional strength, whilst Figs. 34 and 35 show the *Tie-Rod* construction, and Fig. 36 the *Outer Pole* type, of which more will be said below.

A study of these and other designs has led the writer to draft the following specification of a modern multiphase alternator :—

Type.—Three-phase in preference to two-phase, as being

more generally convenient and giving a greater output from a given-sized carcase.

Connections.—Star connection in preference to mesh, this being the usual method of connecting motors and lamps. The stress on the insulation is also less than two-thirds what it would be with mesh connection, whilst with the mesh connection any deviation of E.M.F. from a sine curve may set up internal currents.

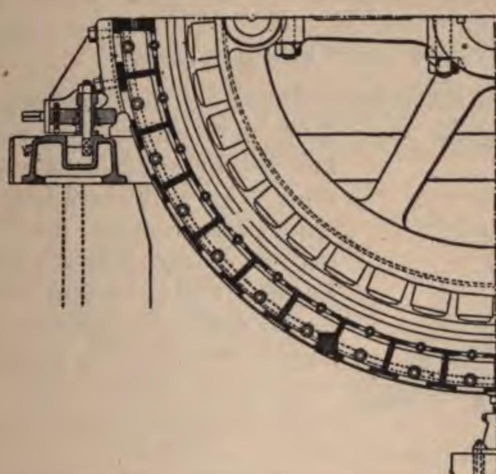


FIG. 37.—Adjustable Foot Bracket to support Armature Ring of Ganz Alternator.

Number of slots.—Two slots per pole per phase. One slot is cheaper to wind, but with two slots the curve approximates more closely to a sine curve. Three slots are too expensive.

Shape of slot.—Half-open tunnel or parallel slot in preference to tunnel, because former-wound coils can then be employed. Tunnels require the coils wound in place, and there is liability of armature leakage across the strip of metal at top.

Poles.—Magnet cast steel of ample area, so that magnetic line density, and therefore the exciting current, can be kept as low as possible. A coil on every pole.

Shape of pole.—Round if possible, but at any rate oval, so as to reduce the leakage from pole to pole and

enable the winding to be edgewise bare copper strip.

Pole-pieces.—Laminated, with the pole-tips cut back and reduced in area so as to give approximation to sine curve and reduce armature leakage to a minimum.

Damping-coils.—Copper castings between the pole-tips and connected up at end. Amortisseur coils necessitate holes in the pole-pieces, and are therefore more expensive to arrange.

FLYWHEEL.

The continuous-current dynamo has usually a flywheel keyed alongside it to help the even-turning movement, but in the case of the alternator there is no separate flywheel. Alternator construction has therefore become closely bound up with the flywheel, and the design of the latter is arranged accordingly. For example, the wheel is generally provided with double arms, not for extra strength but to provide space for the pole bolts.

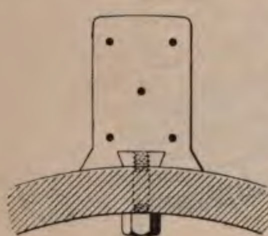


FIG. 38.

The most usual method of fixing an alternator flywheel to the shaft is to cast the boss in sections and have two very heavy shrink rings round the boss. If properly done no key is required. The rim of course must be continuous, as the poles are necessarily pitched very close together. One of the best methods of fastening the two halves of the flywheel is by the link and lug joint, and if the rim is of good depth the shrinking links may be circular and therefore easily and accurately fitted.

In some very large 5,000 k.w. three-phase Westinghouse alternators for the Manhattan Elevated Railway arms are dispensed with altogether, the rim and the boss being secured together by two webs of riveted rolled-steel plates, see Fig. 31.[1]

[1] It is interesting to note that the flywheel rim of the Korting alternators is made extra wide so as to cover the Dettmar electro-magnetic brake which forms part of the bottom half of the armature ring. This electric magnet forms a very convenient method of applying an artificial load for synchronising or testing.

Adjustment of Air-gap.

The accurate adjustment of air-gap is one of the problems of alternator construction, because the diameters of alternator armatures (15 or 20 feet) are great as compared with the usual air-gap, of say $\frac{5}{16}$ inch. An expensive armature construction may really be the means of cheapening the machine as a whole, by enabling a short air-gap to be employed, because it means less field copper, less leakage, and therefore a lower voltage drop.

A common method of adjusting the gap is by means of screw wedges under the feet and at the bottom of the armature ring. In some cases the feet of the armature ring are cast separately, and provided with both vertical and horizontal adjusting screws as in Fig. 37.

In the Siemens and Halske machine the air-gap is adjusted by a special fitting provided with adjustable rollers on which the bottom of the armature ring is supported. The rollers can be raised or lowered separately or together; if adjusted together they give a vertical displacement, whereas if one only is moved the displacement is horizontal.

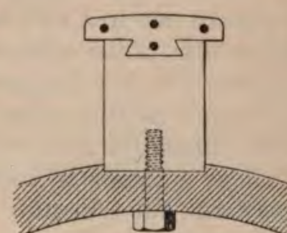


Fig. 39.—Laminated Tips cast into Steel Pole.

One way of approximating to an equality of air-gap is by what may be called the "Brown" construction, shown in Fig. 30. The armature ring has arms down each side, which rest on trunions, and the periphery of the armature ring is fitted with a rack so that it may be barred round and the coils of the bottom half, examined and cleaned.[1] At Frankfort, where this type of machine is employed, the motor for barring the flywheel is also used for barring round the armature ring.

Tie-rod Construction.

When the Schuckert machine, having radial tie-rods to

[1] The only way to clean a dynamo machine effectually, whether it is an alternator or a dynamo, is by means of an air-blast. For high-voltage machines the air-blast is especially necessary, as a damp cobweb between the coils may be dangerous. Where a station is fitted with air-blast static transformers a pipe can be connected temporarily to the air chamber, or where such transformers are not used a small centrifugal fan can be belted up to the alternator shaft.

give stiffness to the frame, was shown at the Paris Exhibition, it came in for a good deal of adverse criticism. The light built-up armature ring, instead of a massive casting, was a distinct innovation, but when one considers that the problem is very similar to that of bridge construction, the wonder is that the built-up framing was not tried at an earlier date.

In alternators of comparatively small diameter it is easy to give such a section to the armature ring that there is practically no deformation when set up in position. As the diameter increases, however, it becomes more difficult to ensure that the alternator ring shall keep to an exact circle when erected vertically, more especially as castings of very large diameter are usually bored out, machined, and wound whilst in the horizontal position.

When a round body of large diameter is brought from an horizontal to a vertical position, any change in form by the action of its own weight is likely to be still further increased by the magnetic pull of the poles. Of course, in the case of alternators driven by vertical spindle turbines, this difficulty does not arise; in fact, a vertical spindle is ideal, as it admits of the umbrella type machine and so does away with all armature deformation. When one comes to think of it there is really no reason why an engine crank-shaft should not be arranged vertically; it works extremely well for dock pumping, and much of the weight on the footstep could be relieved by placing the armature out of centre and so giving a magnetic pull upwards, or possibly a special electro-magnetic device could be fitted for the purpose. The A.E.G. design for the construction is shown in Figs. 34 and 35, and it will be noticed that the tie-rods (with right- and left-hand screws) are arranged tangentially with the armature core. When the machine warms up, these rods tighten, and the whole machine is rigid. Such a construction reduces the weight, and therefore the expense of material, carriage, etc. The armature simply consists of core plates (open to the atmosphere on the outer periphery) clamped between standard cast-iron end rings as in the earliest Elwell Parker alternators.

Inner Pole Machine.

The Outer Pole or Niagara[1] type of machine shown in Fig. 36 has the advantage that the flywheel rim can be made practically of any weight, and it is thus very suitable for driving by single-crank, slow-running engines, especially the large gas-engines which are now coming so much to the front. The armature casting is a hub and therefore not liable to deformation, and as the poles are inside the flywheel rim, the pole-bolts are relieved of all stress due to centrifugal force. The armature is also cheaper to construct because less iron and copper are required.

A feature of the International Electrical Engineering Co's. design in Fig. 36 is that the exciter forms part of the armature support, thus economising space and enabling current to be taken off the commutator at one end of the brush spindle, and passed into the collector rings at the other. This does away with all loose connecting cables.

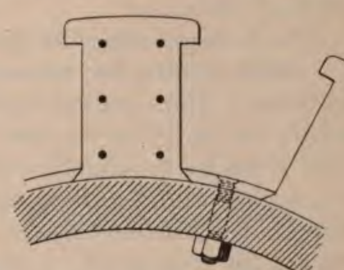


Fig. 40.—Oerlikon Method of holding Laminated Pole.

Fig. 32 shows this idea applied to the exciter of a high-speed machine which has its armature fitted with a racking device to uncover the coils. The exciter very conveniently occupies the extra piece of shaft which is usually wasted.

Poles.

Where the racking device is not provided the poles must be capable of being withdrawn endways so that they may be removed for examination of the armature, and they are sometimes held by vee grooves, as shown in Fig. 38, so as to be easily removable.

Just as in continuous-current machines it was pointed out that an increased line density around the air-gap was a desirable feature, so in an alternator the same holds good.

[1] It is interesting to note that this design—first introduced, by the way, at Cairo by Mr. C. E. L. Brown—has been discontinued in favour of the inner pole type for the latest machines installed in the Niagara station.

Independently of the question of eddy currents therefore, laminated pole-pieces are good practice. The solid pole with laminated tip, as Fig. 39, is better than a pole laminated throughout, because it gives a smaller cross-section and shorter mean turn for field magnet copper, also less leakage between the poles.

When the pole-tips only are laminated they are usually cast into the pole, as shown in Fig. 39, or they may be loose and held on by double-headed keys. When the pole is entirely laminated, it is fastened to the flywheel by means of a square bar passing right through the plates, as shown in Figs. 26, 28 and 29. Fig. 40 shows the method employed for attaching the poles on the Oerlikon type alternators at the Fulham Central Station, and Fig. 41 shows the A.E.G. method of using cotters.

The Ferranti edgewise winding is so entirely superior to any other method for alternators that its use is practically universal. The winding can be made to have no spring by rolling the strip on its outer edge, so that on leaving the rolls it curls up straight away to the radius desired. Of course this rolling method can only be used when the poles are circular. If they are oval in shape (which is most usual) the copper strip must be wound or drawn on to a former to get it to the required shape. The coils rotate at a very high speed and they must be pressed up tight, turn to turn, when placed in position.

In the latest 5,000 H.P. generators at Niagara there are four layers of edgewise strip winding to each field coil (rectangular poles), a clear space being left between the layers for ventilation.

ARMATURE.

The depth of the armature core is more often than not made greater than it need be, purely for mechanical reasons. At high periodicities—that is, 50 and over—the dimension of the pole measured circumferentially is relatively small, and this, of course, only calls for a shallow armature core. To give mechanical strength, however, the core is usually made deep enough for poles giving 25 periods, and at that particular diameter of machine, is so fixed, whatever the periodicity. It is good practice to shorten the magnetic

circuit as much as possible, and for this reason the poles must not be too long nor the armature slots too deep.

Some armatures are bored out after they are built, and on this account the tunnels are *not* cut through, but are punched close up to the inner periphery of the core, so that after being machined the strip of metal at the top of the slot is very thin. With slots or half-open tunnels the armature can be bored out by filling temporarily with wood, but the best method of machining the interior surface is to employ a grinding wheel, which runs backwards and forwards, the armature being moved slightly after each journey. This method has the very great advantage that it does not burr the plates over, and it gives extremely accurate results. Where an armature has tunnels the web at the top of the tunnel should be sawn through, for in boring, the plates get badly burred, and to reduce eddies it is necessary to split up the surface.

FIG. 41.—A.E.G. Method of holding Laminated Pole.

In order to reduce the liability to leakage which must occur when the line density in teeth is pushed to a very high degree, the E. C. Co. punch out vee-shaped slots at the root of each tooth, as shown in Fig. 42 (similar to the Bergmann dynamo). The point of greatest line density is thus transferred to the root of the tooth, where leakage cannot occur to any extent. Further, whilst not interfering very much with direct magnetisation, the notches add somewhat to the reluctance in the cross-magnetisation path, which is, of course, a good feature.

MACHINING SLOTS.

To mill out all the slots of an alternator core, some sixteen feet or so in diameter from the solid, appears at first to be somewhat of an undertaking, yet it is often done, *e.g.*, the 1,600-k.w. 10,000-volt Lahmeyer machines for the Charing Cross and Strand Co. have each some 500 slots about $\frac{3}{8}$ inch × $1\frac{1}{2}$ inch, milled from the solid. Three Collett *and Engelhärt* machines worked on each ring.

There is no doubt that a milled-out slot enables the insulation of the armature coils to be effected in a most thorough manner, and if the core is first dried, the burring of the plates is not serious. Even when the holes are punched out beforehand they generally have to be cleaned out with a milling cutter if they are parallel slots. A drift or file must be used when tunnels are employed, and Fig. 43 shows Mann's slot-cleaning machine, which has been specially constructed for rhymering out such tunnels.

CYCLIC IRREGULARITY.

The question of cyclic irregularity depends mainly on the type of engine. Of course the ideal condition, is the even turning given by a turbine, and from this point of view the more cranks there are on an engine the better. A good deal, however, depends on how the cranks are arranged, and also on the character of the governing. A single crank engine is better than the usual two-crank type at right angles, because there are fewer points of minimum and maximum speed per revolution. In this country the three-crank engine with the alternator coupled on an extension shaft at one end has been extensively adopted, whilst on the Continent very excellent results have been obtained with single-crank tandem compound engines. At Frankfort, for example, there are a number of such units; they all run at the same speed, the cranks being first brought into synchronism by electric bell contacts on the flywheels.

The large 5,000 k.w. machines for the Manhattan Elevated Railway are driven by combined horizontal and vertical steam engines, so as to equalise the turning movement as much as possible.

A great deal can be done to help cyclic regularity by fitting the poles with damping coils. With poles of solid steel it is generally sufficient to make the flange of the magnet coil nearest the pole face of very heavy section gunmetal or copper. In fact, these are often arranged to span from pole tip to pole tip. If the poles are laminated, Leblanc's amortisseur coils, consisting of several solid copper rods let into the face of the pole and short-circuited at the ends, are generally employed. The effect of this low-resistance circuit round the poles or the amortisseur coils is to retard any

shifting of field magnetism across the pole face and so tend to correct variation in angular velocity.[1]

VOLTAGE DROP.

It is important that the voltage drop should be as small as possible, for besides making electric lighting more difficult a large drop is detrimental to the efficient running of induction motors, inasmuch as the torque of such motor varies with the square of the impressed voltage.

The voltage drop of a multiphase alternator is due to ohmic resistance of the copper eddy-currents, armature reaction, armature leakage, and increase of field-magnet leakage from no load to full load.

The eddy-currents set up on the face of an alternator-pole have the effect of demagnetising, and they thus, to a slight degree, increase the drop, and from this point of view a laminated pole-piece is best.

At present the drop is the principal limiting factor of output, being generally about three to eight per cent. with power factor unity, and ten to twenty per cent. with power factor 0·8. Obviously if some good method of compounding could be introduced the designer need not worry about the amount of drop, as he would know that this could be corrected by the compounding, as in the case of an ordinary continuous-current dynamo. This would also result in the output of any given carcase being considerably increased.

Magnetic leakage is the most important factor in alternator design. Of two machines, both of which give the same "*size constant*" d^2l, that which has the largest diameter will, other things being equal, give the best results, because with a given number of poles they are further apart, and, therefore, there is less leakage. On the other hand the expense is greater. Where the periodicity is high the large

[1] The armature current sets up magnetic poles over the core surface, and any change in the relative position of these armature poles and the field poles causes the damping coils to be cut by the shifting armature magnetism, and currents result which oppose this shifting of magnetism. The solid copper rods in the polar surface act as a squirrel-cage winding, and in case of the alternator losing its exciting current the machine will continue to rotate as an asynchronous motor. There is therefore much less liability of breakdown. Gun-metal castings between the poles and partly covering the horns are efficient for damping, but not sufficient to cause the machine to run as a motor.

number of poles required may bring them within a few inches of each other, and there is considerable leakage, even when the air-gap is reduced below the mechanically safe limit. Low periodicities are therefore preferable from the manufacturing point of view.

FIG. 42.

If there is a bridge of metal at the top of the tunnel it soon gets saturated to its utmost limit, and acts practically as an air-gap. At the same time it must be remembered that it is only when the poles are in a certain position that saturation occurs. The lag of the armature current causes it to act on some of the bridges when they are removed from the direct influence of the poles, and, therefore, not saturated. On this account it is best to have half-open tunnels.

We have seen that in dynamos, armature-reaction may be reduced by working at high-line densities[1] on the magnetic material near the air-gap, and by placing reluctance on the cross-field, just in the same way the behaviour of an alternator can be improved by keeping these points in view. The drop, particularly at full load, may be kept down by so designing the machine that there is only a small *difference in the leakage at full load and no load.* The actual amount of leakage is not so material, although, of course, it should be as low as possible, but what is important is that the *difference* should be small.

SHAPE OF CURVE.

There is a commercial limit to which the number of tunnels per pole per phase should be pushed, but obviously the more distributed the winding the better the use which is made of the armature-iron. The more even the line-density and the more nearly does the curve approximate to a sine curve.

The apparent refinement of getting as near to a sine curve as possible repays for any extra trouble, in that it reduces the stresses on the insulation and, to a certain

[1] It may be well to note that as the speed of an alternator is governed directly by the periodicity, it is necessary to have something in hand when fixing on the line-density in the teeth, otherwise the required voltage may not be obtained.

extent, gives a saving of energy. The self-inductance of a circuit smooths out irregularities in the potential curve to a certain extent, but, at the same time, where rotary converters are employed, it is well to take extra precautions to get as near a sine curve as possible.

Good results are obtained by bevelling the tips of the field-poles, so giving a better distribution of magnetic lines.

Tables, etc.—Multiphase Alternators.

Alternators are usually worked at higher peripheral speeds than dynamos, for one thing because the periodicity requires a larger number of poles, and to find room for them large diameters are necessary. As will be seen from Table VI., speeds of over 5,000 feet per minute are common. As a matter of fact well-made shrink-ring jointed cast-steel flywheels may be run at 7,000 feet per minute and still have a fair factor of safety. Cast iron is limited to about 5,000 feet per minute. The weight of a flywheel alternator being relatively so very great, it is only possible to keep the shaft within reasonable dimensions by reducing the distance between the bearings, for the deflection of a shaft is roughly proportional to the cube of the distance between the bearings. On this account the necessary weight is usually obtained by making the magnet wheel serve as the flywheel, and velocity squared is obtained by increasing the diameter to its utmost limit. With a built-up construction as shown in Fig. 31, a peripheral speed of as high as 8,000 feet per minute can be attained.

Table VII., which is deduced from Table VI., gives coefficients as in the case of the multipolar dynamos, and it will be seen that they vary considerably, *e.g.*, applying the Steinmetz formula to alternators the figure appears to vary from two to eight, and in the "*output equation*"

$$\text{Watts} = \text{coefficient} \times n \times d^2 \times l,$$

the figure varies in somewhat the following manner :—

	Coefficient for output equation.
For the largest flywheel alternators	·03
" large alternators	·02
" medium size alternators, say 300 k.w. ...	·01
" small alternators	·005

Table VIII. gives the proportions of a line of two-phase alternators for various values of d^2l. It will be noticed that as the machines get larger in diameter, the length of the core tends to remain at about 10 per cent. of the diameter. As a matter of fact, in getting out a new machine many designers arrange the size so as to get a circular section-pole. The particulars given in Table VIII. are for two-phase machines on a non-inductive load.

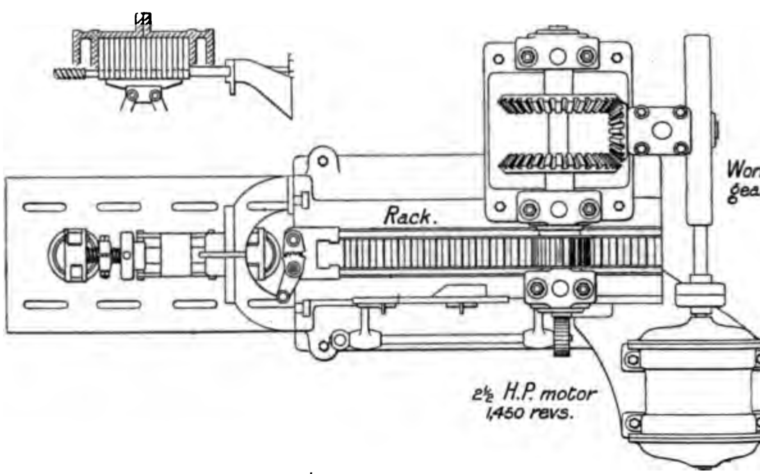


FIG. 43.—Mann's Slot-clearing Machine.

The percentages of the various items which go to make up the material for a machine say 7 feet diameter, are given below—

Materials—	Percentages of total cost of material.
Armature copper	2 per cent.
Field copper	9 ,, ,,
Armature stampings	13 ,, ,,
Field poles	8 ,, ,,
Insulating materials	5 ,, ,,
Armature shell, flywheel, shaft, bearings ...	42 ,, ,,
Miscellaneous	21 ,, ,,

I have to thank the various firms, including Madame Dunod and Co., the publisher of *l'Électricité a l'Exposition de* 1900, who have kindly supplied several of the drawings illustrating this paper.

TABLE II.

Coefficients, etc., Deduced from Table I.

Maker.	K.W.	Revs. per Min.	Dia. over Armature.	Length of Armature Core.	Diameter Squared in Ins. Multiplied by Length in Inches.	Peripheral Speed. Feet per Minute.		Coefficient by Steinmetz Formula :— $\frac{\text{Dia.}'' \times \text{Length}''}{\text{K.W.}} =$	Coefficient from Formula :— Watts $= k \times n \times l \times d^2$. k = Coefficient. n = Revs. per Minute. l = Length of Arm. Core. d = Dia. of Armature.	Area Pole to Area of Armature.	Area Polar Surface to Area of Armature.
			Inches.	Inches.		Arm.	Com.		Inches.	– to one.	– to one.
Siemens Bros. & Co.	1,530	200	108	21	244,000	5,320	3,420	1·4	·032		
Siemens & Halske	1,000	95	100	20·8	208,000	2,640	2,050	2·0	·052	2·1	3·3
International Electrical Engineering Co.	800	85									
Société Alsacienne de Constr. Mecaniques	750	70	152·5	20	465,000	2,750	2,750	3·9	·024	2·98	5·1
Schuckert & Co.	750	83½	101	16	163,000	2,160	1,460	2·1	·058		
General Electric Company of Creil	700	120	90	20	162,000	2,860	1,970	2·5	·038	2·32	4·25
Societa Esercizeo Bacini de Genes	500	160	116	14	188,400	4,750	1,650	3·1	·017	0·95	2·0
Decauville Co.	400	75	120	14	202,000	2,320	2,260	4·1	·028	2·2	5·4
Postal-Vinay	350	90	64	24	98,250	1,480	1,100	4·2	·041		
Mather & Platt	350	105	80	18·4	117,500	2,160	1,950	4·0	·03	3·1	3·1
Lahmeyer & Co.	350	94	96	16	148,000	2,320	1,920	4·2	·027	1·04	2·4
Ernest Scott and Mountain	330	90	60	20	72,000	1,420	990	3·6	·051	1·86	2·3
De Maubeuge Blast Furnace Co.	280	120	94	20	177,000	2,920	2,230	6·4	·014		
Alioth	225	280	60	14	50,000	4,350	2,740	3·6	·017	1·64	3·8
Société l'Eclairage Electrique	200	110	80	14	90,000	2,260	1,360	5·4	·021	2·9	5·1

TABLE IV.

Approximate Particulars of a Line of Multipolar Dynamos.

Size Constant $d^2 l$, both in inches.	Ratio of Length of Armature Core to Diameter.	Diameter of Armature Core.	Length of Armature Core.	Suggested Number of Poles.	Kilowatts.	Revolutions per Minute.	Approximate Weights in Lbs.					
							Armature Copper.	Field Copper.	Commutator Copper.	Armature Core.	Magnet Poles.	Yoke.
		Inches.	Inches.									
4,200	0·45	21	9½	4	40	650	150	500	100	500	800	3,000
5,750	0·42	24	10	4	50	600	200	600	125	700	1,000	3,500
7,600	0·39	27	10½	4	60	550	250	700	150	1,000	1,150	4,000
10,000	0·37	30	11	4	75	500	300	800	175	1,200	1,300	4,500
12,300	0·35	33	11½	4	90	450	350	900	200	1,400	1,450	5,000
15,600	0·33	36	12	4	125	400	400	1,000	225	1,600	1,600	6,000
23,000	0·31	42	13	4	175	350	550	1,100	350	1,900	1,800	6,500
32,000	0·29	48	14	6	225	300	700	1,200	500	2,300	2,500	7,000
44,000	0·28	54	15	6	275	250	900	1,400	550	3,000	3,500	8,000
50,000	0·27½	57	15½	6	300	225	1,150	1,600	700	3,500	4,500	8,500
58,000	0·27	60	16	8	325	200	1,350	2,000	850	4,000	5,500	9,000
74,000	0·25½	66	17	8	400	175	1,500	2,500	1,000	5,000	6,000	10,000
94,000	0·25	72	18	10	500	150	2,000	3,500	1,250	6,000	8,000	12,000
145,000	0·24½	84	20½	10	600	125	2,500	5,000	1,500	8,000	10,000	15,000
215,000	0·24	96	23	12	800	110	3,000	7,000	1,750	11,000	14,000	20,000
300,000	0·23½	108	25½	14	1,000	100	4,000	9,000	2,000	15,000	18,000	25,000
405,000	0·23¼	120	28	16	1,200	90	5,000	12,000	2,500	20,000	22,000	30,000

TABLE V.

Approximate Particulars of a Line of Bi-polar Dynamos.

Size Constant $d^2 l$, both in inches.	Ratio of Length of Armature Core to its Diameter.	Diameter of Armature Core.	Length of Armature Core.	Kilowatts.	Revolutions per Minute.	Approximate Weight in Lbs.				
						Armature Copper.	Field Copper.	Commutator Copper.	Armature Core.	Magnets.
		Inches.	Inches.							
440	1·28	7	9	5	1,200	25	125	12	80	100
600	1·29	7¾	10	7½	1,100	32	170	16	110	120
800	1·29	8½	11	10	1,000	40	210	20	150	150
1,030	1·29	9¼	12	12½	900	50	270	25	200	180
1,270	1·28	10	12¾	15	850	60	320	30	250	210
1,700	1·27	11	14	18	750	75	390	35	320	250
2,240	1·29	12	15½	22	700	100	480	45	400	300
2,830	1·29	13	16¾	27	650	135	550	60	520	350
3,540	1·28	14	18	35	600	200	750	75	640	410
4,350	1·28	15	19¼	45	550	280	950	100	760	470

TABLE IV.

Approximate Particulars of a Line of Multipolar Dynamos.

Size Constant $d^2 l$, both in inches.	Ratio of Length of Armature Core to Diameter.	Diameter of Armature Core.	Length of Armature Core.	Suggested Number of Poles.	Kilowatts.	Revolutions per Minute.	Approximate Weights in Lbs.					
							Armature Copper.	Field Copper.	Commutator Copper.	Armature Core.	Magnet Poles.	Yoke.
		Inches.	Inches.									
4,200	0·45	21	$9\frac{1}{2}$	4	40	650	150	500	100	500	800	3,000
5,750	0·42	24	10	4	50	600	200	600	125	700	1,000	3,500
7,600	0·39	27	$10\frac{1}{2}$	4	60	550	250	700	150	1,000	1,150	4,000
10,000	0·37	30	11	4	75	500	300	800	175	1,200	1,300	4,500
12,300	0·35	33	$11\frac{1}{2}$	4	90	450	350	900	200	1,400	1,450	5,000
15,600	0·33	36	12	4	125	400	400	1,000	225	1,600	1,600	6,000
23,000	0·31	42	13	4	175	350	550	1,100	350	1,900	1,800	6,500
32,000	0·29	48	14	6	225	300	700	1,200	500	2,300	2,500	7,000
44,000	0·28	54	15	6	275	250	900	1,400	550	3,000	3,500	8,000
50,000	$0·27\frac{1}{2}$	57	$15\frac{1}{2}$	6	300	225	1,150	1,600	700	3,500	4,500	8,500
58,000	0·27	60	16	8	325	200	1,350	2,000	850	4,000	5,500	9,000
74,000	$0·25\frac{1}{2}$	66	17	8	400	175	1,500	2,500	1,000	5,000	6,000	10,000
94,000	0·25	72	18	10	500	150	2,000	3,500	1,250	6,000	8,000	12,000
145,000	$0·24\frac{1}{2}$	84	$20\frac{1}{2}$	10	600	125	2,500	5,000	1,500	8,000	10,000	15,000
215,000	0·24	96	23	12	800	110	3,000	7,000	1,750	11,000	14,000	20,000
300,000	$0·23\frac{1}{2}$	108	$25\frac{1}{2}$	14	1,000	100	4,000	9,000	2,000	15,000	18,000	25,000
405,000	$0·23\frac{1}{4}$	120	28	16	1,200	90	5,000	12,000	2,500	20,000	22,000	30,000

TABLE V.

Approximate Particulars of a Line of Bi-polar Dynamos.

Size Constant $d^2 l$, both in inches.	Ratio of Length of Armature Core to its Diameter.	Diameter of Armature Core.	Length of Armature Core.	Kilowatts.	Revolutions per Minute.	Approximate Weight in Lbs.				
						Armature Copper.	Field Copper.	Commutator Copper.	Armature Core.	Magnets.
		Inches.	Inches.							
440	1·28	7	9	5	1,200	25	125	12	80	100
600	1·29	$7\frac{3}{4}$	10	$7\frac{1}{2}$	1,100	32	170	16	110	120
800	1·29	$8\frac{1}{2}$	11	10	1,000	40	210	20	150	150
1,030	1·29	$9\frac{1}{4}$	12	$12\frac{1}{2}$	900	50	270	25	200	180
1,270	1·28	10	$12\frac{3}{4}$	15	850	60	320	30	250	210
1,700	1·27	11	14	18	750	75	390	35	320	250
2,240	1·29	12	$15\frac{1}{2}$	22	700	100	480	45	400	300
2,830	1·29	13	$16\frac{3}{4}$	27	650	135	550	60	520	350
3,540	1·28	14	18	35	600	200	750	75	640	410

E-PHASE ALTERNATORS

Diameter of Shaft	Section of Pole.	Length of Coil.	Polar Surface.
c.m.	sq. c.m.	c.m.	sq. c.m.
60	314	20	670
50	600	20	900
63	500	10	450
60	50	15	400
45	320	16	420
45	283	18	525
58	450	10	675
60	50	20	220
55	10	25	720
[illegible]	600	24	675
[illegible]	100	15	315
[illegible]	6	16	500
5	[illegible]	20	370
[illegible]	39	18	147

TABLE VII.

COEFFICIENTS, ETC., DEDUCED FROM TABLE VI.

Number.	MAKER.	K.W.	Revs. per Min.	Dia. over Poles.	Length of Armature Core.	Diameter Squared Multiplied by Length.	Peripheral Speed over Poles. Feet per Minute.	Coefficient by Steinmetz Formula :— $\frac{\text{Dia.}'' \times \text{Length}''}{\text{K.W.}} =$	Coefficient from Formula :— Watts = $k \times n \times l \times d^2$. k = Coefficient n = Revs. per Minute. l = Length of Arm. Core. d = Diameter over Poles.	Area Pole to Area of Armature. — to one.	Area Polar Surface to Area of Armature. — to one.
				Inches.	Inches.	Inches.			Inches.		
1	Helios	3,000	$71\frac{1}{2}$	314	13·4	1,320,000	5,850	1·4	·0316	·85	1·81
2	Siemens & Halske ...	2,000	$83\frac{1}{2}$	235	23·7	1,320,000	5,100	2·78	·0183	·67	1·0
3	French Thomson-Houston	1,000	75	140	13·5	348,000	2,750	1·89	·053	·74	·74
4	Schneider & Co. ...	1,400	71·5	$251\frac{1}{2}$	10	629,900	4,718	1·78	·0306	·57	·86
5	Oerlikon	1,340	94	197	12·2		4,820			·6	·6
6	Ganz & Co.	1,200	125	163	12·2	324,000	5,330	1·66	·0296	0·718	1·33
7	Société à l'Eclairage Electrique	1,200	79	224	19	950,000	4,620	3·6	·016	·93	1·4
8	International Electrical Engineering Co. ...	1,000	$83\frac{1}{3}$	216	8	373,000	4,680	1·73	·032	·5	·74
	Schukert & Co.	850	$83\frac{1}{2}$	216	15·8	735,000	4,680	4·0	·0138		
9	Kolben & Co.	825	94	216	15·8	735,000	5,280	4·2	·0118	·75	1·1
10	Electrique Hydraulique	800	94	223	11	546,000	5,480	3·06	·0155	·75	·75
11	Fives-Lille	800	79	235	10	552,000	4,860	3·0	·018	·75	1·1
12	M. A. Grammont ...	600	$93\frac{3}{4}$	197	11	426,000	4,810	3·6	·015		
13	General Electric Co. London	300	$93\frac{3}{4}$	$168\frac{1}{2}$	5·3	180,000	4,000	2·98	·021	·4	·45
14											

TABLE VIII.

Approximate Particulars of a Line of Two-phase Alternators.

Size Constant $d^2 l$.	Length of Armature Core in Percentage of its Diameter.	Internal Diameter of Armature Core.	Length of Armature Core.	Number of Poles for a Periodicity of 50.	Kilowatts as Two-Phase on a Non-Inductive Load.	Revolutions per Minute.	Approximate Weight in Lbs.			
							Armature Copper.	Field Copper.	Armature Core.	Poles.
Inches.		Inches.	Inches.							
12,300	16¾	42	7	12	45	500	85	480	1,300	900
17,200	15½	48	7½	14	50	430	100	550	1,500	1,000
23,400	14¾	54	8	14	60	430	110	600	1,700	1,200
30,600	14	60	8½	16	70	375	140	650	1,900	1,500
39,200	13½	66	9	16	85	375	165	750	2,200	1,700
49,200	13	72	9½	18	100	333	200	850	2,500	2,000
70,500	12	84	10	20	110	300	250	950	3,000	2,200
102,000	11	96	10½	24	140	250	360	1,100	3,500	2,500
128,000	10¼	108	11	30	170	200	500	1,600	4,000	3,000
173,000	10	120	12	40	200	150	600	2,100	5,000	4,000
226,000	10	132	13	48	250	125	730	2,700	6,000	5,000
290,000	10	144	14	56	310	107	860	3,200	8,000	6,500

The PRESIDENT: Gentlemen, it is rather late in the evening to begin the discussion. We must remember that the authors of these papers have sacrificed themselves in our behalf by reading the papers so very shortly. If you look at the papers you will find that to-night we have practically gone through one hundred pages of our Journal. Both the papers are full of detail and important matter. Any one who wishes to discuss the papers ought to read them carefully, because there is an immense amount of information in them that has not been read to-night. The President.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Members.

David Sing Capper.
Albert Neumann Connett.
Walter Bernard Hopkins.
John Beaumont Mitchell.
Charles Remington.
Calvin W. Rice.
Norman Scott Russell.
Guy Lutley Sclater (Com. R.N.)
Charles Felton Scott.
Harold Babbitt Smith.
Charles Weiss.

Associate Members.

James Aitken.
Benjamin John Day.
Colin McKenzie Gardner.
Charles A. Gillin.
George Frederick Gower.
Tom Welbeck Graves.
Edward Stanley Harpham.
Charles Robert Heath.
Frederick Hugh Rothes Neville.
Richard Pape.
Hubert Edward Rogers.
Oliver Cromwell Spurling.
Cecil Strafford.
Frank Walter.
Ludwig Hermann Wilms.

Associates.

William Roger Anderson.
Ernest Brook.
E. A. T. W. Clifford.
James Coxon.
George Wills Cripps.
Robert Napier Cunningham.
Harry Curphey.
Robert Cuthbert.
Damodar Ganesh Dani, B.Sc., F.C.H.
Frank George Evans.
Harold R. G. Forster.
Frederick Alwyn Haigh.
John Cruttall Jenner.
John Kirkwood.
W. A. R. Knight.
Robert Jaffray Nicholson.
William Edward Reath,
Geo. Rob. Wesley Roberts.
Wm. Morrish Selvey, Wh. Sc. A.R.C.Sc.
Thos. Reginald Stancombe.
Frederick Othniel Steed.
Robert Steel.
John William Turner.
Harold Walker.

Students.

James Emile Andrews.
Friend Hartley Beal.
Fred M. Bray.
Roy Apted Broad.
Alec Burrowes, 2nd Lieut. R. G. A.
Robert Harold Chalk.
Geo. Augustin Cladingbowl.
G. B. G. Cleather.
Alfred Craven.
C. A. Henry Edwards.
Roy Remington Elliott.
Arthur Edward Flynn Fawcus.
David Derwent James Fawcus.
Patrick Anthony Gibney.
John Scear Gibson.
Arthur Allan Gomme.
Robert Alfred Ives.
Stanley Jones.
C. Herbert Lange.
James Burne Leefe, 2nd Lieut. R. G. A.
Stanley Harris May.
Alexr. Duncan Melville.
Leopold Geo. Esmond Morse.
Charles Sidney Perry.
Lewis W. Phillips.
John Morgan Griffith Rees.
Cecil Robinson.
Charles Wesley Rycroft.
Harry Robert Speyer.
Harold Stanley Taylor.
Wilfrid Stephen Taylor.
Richard W. R. Twelvetrees.
Frederick Wakeman.
Harry Whitworth.

The Three Hundred and Eighty-eighth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, February 12th, 1903—Mr. JAMES SWINBURNE, President, in the chair.

The minutes of the Ordinary General Meeting held on February 5th, 1903, were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that their names should be suspended in the Library.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Associate Members—

Walter Ainscough.
Alfred N. Hazlehurst.
Francis H. Merrit.
John S. Plumtree.
Henry F. J. Thompson.
John C. A. Ward.
Ernest T. Williams.
Adolf Schoder.

From the class of Students to that of Associates—

Charles E. Gunner.
Mahmoud Samy.

Messrs. F. C. Knowles and O. C. Spurling were appointed scrutineers of the ballot for the election of new members.

Donations to the *Library* were announced as having been received since the last meeting from Mr. E. Garcke ; to the *Building Fund* from Messrs. F. W. Clements, W. P. Digby, V. M. Gill, M. M. Gillespie, W. W. Strode, G. Walsh, and C. E. Wilson ; and to the *Benevolent Fund* from Messrs. H. J. Glynn and M. M. Gillespie, to whom the thanks of the meeting were duly accorded.

The PRESIDENT : The Council, in accordance with a suggestion that has been made, ask any members to put forward the names of Members, Associate Members, or Associates they would like to see on the Council, and any suggestions that are sent in before the 12th of March will be considered by the Council. The idea is that the Council might by some chance pass over somebody who ought to be on the Council, and therefore if any of the members suggest a candidate who they think ought to be on the Council, if they will put his name forward, it does not mean that the nomination will be made, but that the Council will consider that name when they are preparing the list of nominations. The President.

We will now begin the discussions on the papers of Mr. Esson and *Mr. Kilburn Scott*. Will Professor Carus-Wilson open the discussion?

Carus-Wilson.

Prof. CARUS-WILSON: With reference to the question of commutation on page 404 of his paper, Mr. Scott says, "Designers appear to be practically agreed that to get sparkless commutation and a minimum zone of movement of the brushes, there must be a certain relationship between the ampere turns required for the air-gap at full load and the cross ampere turn of the armature." He then gives tables, illustrating this statement, obtained from modern standard railway and lighting generators. This view of the sparking question has been accepted as a working hypothesis for many years, ever since Mr. Esson brought it before this Society; but it has given place to other views, notably to that held by American designers. The old view is illustrated by the diagram in Fig. A. This shows the surface of the armature with the fields developed in the usual way, and curves giving the magnetisation due to the fields and armature reaction, also a curve of magnetisation due to the combination of the two with the brush unshifted. The old view amounts to this: that in order to get sparkless commutation, the

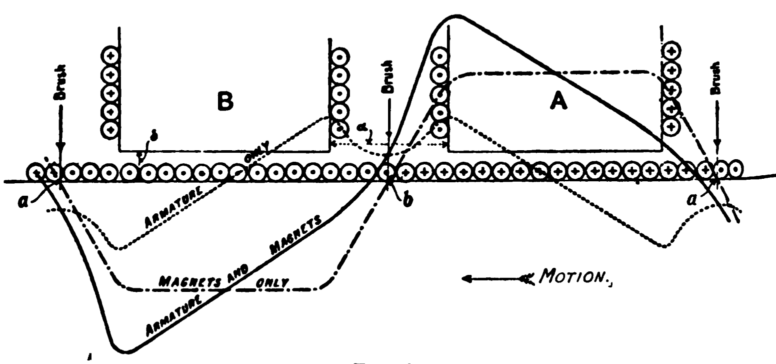


FIG. A.

magnetisation produced by the armature at the pole-tips must be less than that produced by the magnets at the pole-tips.

Now in the 200-kilowatt machine referred to by Mr. Scott, the effect of the armature at the poles is about equal to that due to the field. Thus from the old point of view it is, as Mr. Scott says, surprising that it is possible to get sparkless commutation with the field at the pole due to the magnets completely wiped out by armature reaction. We are not, however, concerned with what happens at the poles, but with what goes on under the brush, and any formula or rule which gives us a relation between the fields produced by armature and magnets under the poles, really does not help us at all. We want to know what it is that goes on under the brush. The field under the pole-tip may be completely wiped out; but so long as we have the right state of affairs under the brush, we may get sparkless commutation. So that this old view of the sparking question, based upon what happens under the poles, is absolutely no guide whatever. The American way of looking at the question is better. It is this: they consider what they call the reactance voltage; that is to say, instead of troubling their heads about what goes

on under the pole-tip, they measure the tendency of a coil to spark by what they call the reactance voltage. Taking that 200-kilowatt machine referred to by Mr. Scott, the reactance voltage per coil under the brush is about 8 volts, that is, they take this as a measure of the tendency of the coil to spark. That is a great improvement as far as it goes. But it fails in this respect: that they then go on to compare this reactance voltage with the average voltage per coil; that is, the voltage between the brushes divided by the number of coils between the brushes. Then it is put in this way: that the reactance voltage must bear a certain relation to the average voltage. In the case of the 200-kilowatt machine already referred to, the average voltage is about 7 volts, and the reactance voltage 8 volts, and the view is that there must be more or less of an equality between these two; that is, if the reactance voltage is high the average voltage must be high, and *vice versa*. Prof. Carus-Wilson.

Now I consider this to be an entirely delusive idea, and that the relation of these two voltages, although it may in a certain type of machine give some indication as to whether the machine is properly designed, really does not indicate in the least what is going on under the brush, because the average voltage is a purely imaginary thing, and does not represent anything going on under the brush that counteracts the tendency to spark as measured by the reactance voltage. The only thing that we can go by is the actual field in which the brush is placed. Fig. A shows that the field produced by the armature under the brush is always of the wrong sign for commutation; and the amount of this field depends practically upon the interpolar distance, a in the figure. As a matter of fact the sparkless running hardly depends at all upon the width of the air-gap, δ, it depends mainly upon this distance a. In the 200-kilowatt machine the induction under the gap due to the magnets is 8,000 lines per square centimetre, the induction due to armature reaction under the pole-tip is 8,600 lines, and the induction under the brush due to armature reaction is equal to 2,600 lines per square centimetre, and is of the wrong sign for commutation; that is to say, so far from having the brush in a field of the right sign for commutation, there is actually a field of 2,600 lines per square centimetre of the wrong sign for commutation.

Comparing the 200 and 250-kilowatt machines, in the former the reactance voltage is 8 and the average voltage is 7; in the latter the reactance voltage is 5 and the average voltage is 5; so that from this point of view the two are practically equal as regards sparking. Yet we find that while in the 200-kilowatt machine there is an induction under the brush of 2,600 lines, in the other machine the induction is just half of that, and the conditions of commutation consequently much better. The reason of the difference is mainly that the interpolar distance in the first machine is only 21 centimetres, while in the second machine it is 32 centimetres.

What I want to urge is this, that this American method of estimating the commutating conditions of a machine by comparing the reactance voltage with the average voltage, though a step in the right direction, does not go far enough. We want to compare the

Carus-
on.

reactance voltage, or the tendency to spark, with the actual magnetic condition under the brush. It is remarkable that sparkless commutation can be effected with the brushes in a strong field of the wrong sign for commutation; if it were not for the use of the carbon brush we could not possibly get the results that we do. The distribution curves of any of these generators or motors show that the brush is commutating sparklessly in conditions which one would have believed to be absolutely impossible. I have some slides showing the results of tests made on a railway motor.

Fig. B gives the magnetisation curves, showing the strong field

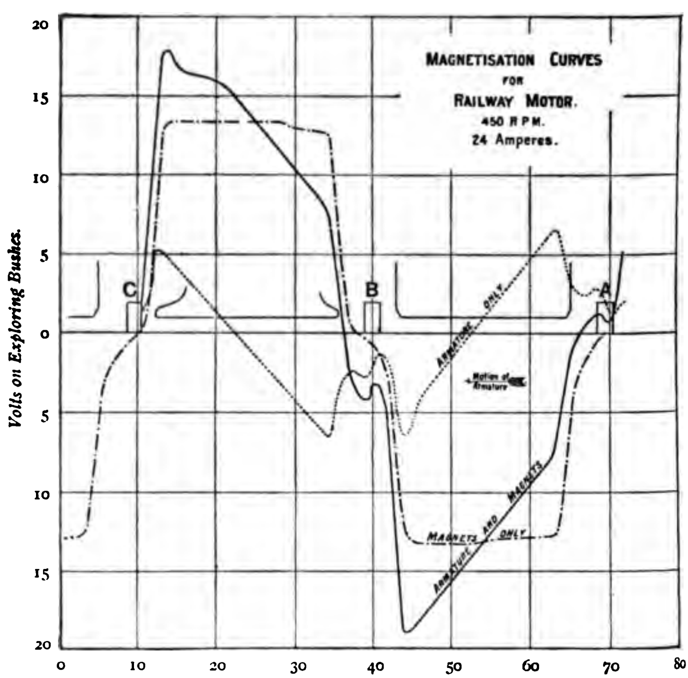


FIG. B.

under the brush due to armature reaction, of the wrong sign for commutation. This motor was being run as a generator, so that the sign of the respective fields are those for a generator. There is a great deal yet to be done on this question of commutation, and if I might throw out a hint to those who have laboratories and money to do what they like with, it would be to investigate with a Duddell's oscillator the condition of current reversal under a brush during the operation of commutation.

Eborall.

Mr. A. C. EBORALL: I would first of all like to thank Mr. Esson and Mr. Scott for their papers; such papers, together with the discussions upon them, bring out many interesting and useful points,

and are, therefore, greatly appreciated by designers and others interested in the subject. Mr. Eborall

The first point I would like to refer to briefly to-night is in connection with the question of the "output-coefficient" of standard machines, which has been dealt with by both authors. I have never been able to make much use of the Steinmetz formula given by Mr. Scott, and consider that its utility is limited strictly to standard lines of machinery, all of the same type, and designed upon similar lines with regard to electrical and magnetic constants. Hence, I agree with Mr. Scott that the better known output rule, which takes the speed into account, is to be preferred for preliminary calculations.

I should like to draw your attention to the fact that, in a well-designed series of machines with similar constants, the value of the

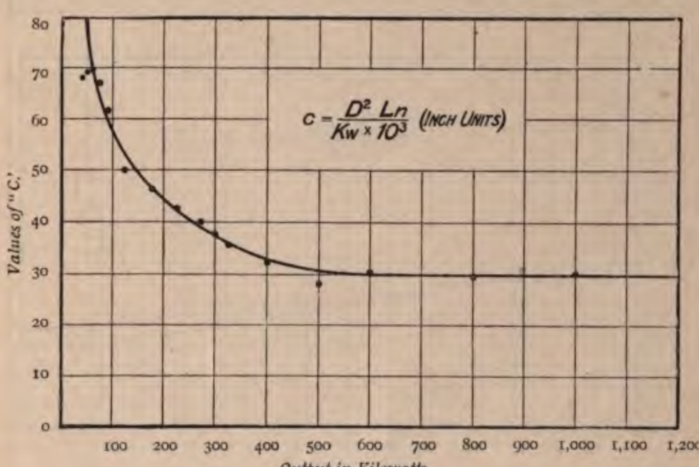


FIG. C.

output coefficient varies in a perfectly regular way, according to the size of the machine. Thus, for instance, Fig. C shows the value of the coefficient for a standard series of modern direct-current dynamos, all built to a certain standard specification; the points lying off the curve, marked by crosses, belong to older types, abandoned because the machines in question had bad "running values." Fig. D shows a similar curve, drawn out for the standard series of machines given by Mr. Scott in Table IV.; in this case, as will be seen, certain sizes might be improved by slightly altering the dimensions or the ratings. Similar curves can be constructed for alternators and induction motors, and are of considerable use to the designer for preliminary calculations.

Referring to Mr. Scott's standard specification for polyphase generators, I would say that, in my opinion, it is not possible to lay down hard and fast rules governing the construction, as Mr. Scott has done, for most cases have to be decided upon their own merits, *at any rate with* very large machines. For instance, consider the

Thorall. number of armature slots—in a large machine it will rarely happen that two slots per pole per phase will be enough, and the advantages in employing a larger number in the way of getting rid of harmonics in the E.M.F. wave, and in diminishing armature leakage and noise, will far more than compensate for the slight additional expense. Again, although I am personally in favour of former-wound coils as a rule, yet for certain cases hole windings are superior.

I would next like to correct Mr. Scott's statement that a two-phase generator requires 25 per cent. more armature copper than a three-phase machine. As a matter of fact, as Mr. Esson states, there is practically no difference in the two types,[1] a standard carcase giving the same output, whether as two-phase or three-phase, with the same heating and pressure drop. It is otherwise, however, with two-phase motors, as these are not only larger, but they are some-

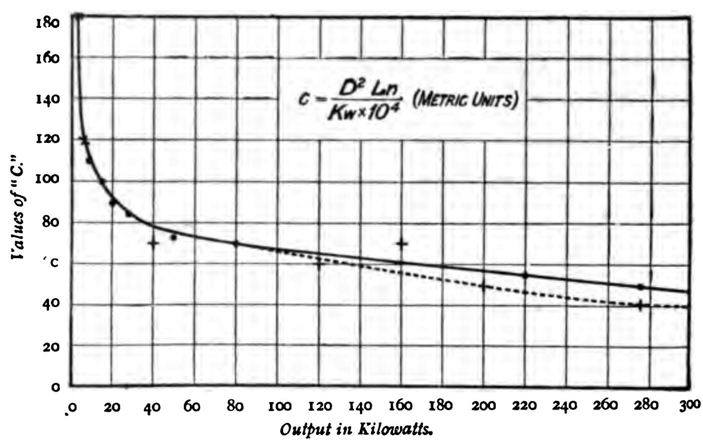


FIG. D.

what inferior to three-phase motors—for instance, a carcase for a 100 H.P. motor must be rated at 90 H.P. for a two-phase motor, the efficiency and power-factor of the latter being about 1 per cent. less, and the overload capacity, before falling out of step, 25 per cent. less.

In reference to high-speed alternators referred to by Mr. Scott, at a given speed the low-frequency alternator has fewer poles than the high-frequency machine; the latter has many poles, and hence the higher the speed the better. As a matter of fact, if there is one fact more certain than any other, in connection with alternator design, it is that the higher the speed the better will be the machine, quite irrespective of the frequency or anything else. For the pressure

[1] Of course, owing to the greater variations in the strength of the armature reactive flux, the armature reaction is relatively a little greater (about 6 per cent.) with a two-phase generator than with a three-phaser. But this small difference, does not, in practice, affect the size or cost of the two-phase machine.

regulation of the high-speed machine is, of course, much better, while the parallel running is much better, firstly, because the cyclic irregularity of the high-speed engine is less, and, secondly, because, with a given cyclic irregularity, the phase displacement of the E.M.F. waves of the machine in parallel is less, on account of the larger pole pitch of the high-speed machines. Mr. Eborall.

In connection with the pressure regulation of alternators, while agreeing with Mr. Scott that it is of importance that the magnetic leakage of the field system should vary as little as possible from no load to full load, if a small drop of pressure is required, yet this is certainly not the most important point to be observed. Of greater importance is the proper ratio of field ampere turns to armature ampere turns, and also the reduction of the armature leakage to a minimum, the proper length of the air gap, and the saturation of the pole shoes and armature teeth. It is only by attending to all these matters that the pressure drop can be kept within reasonable limits, especially on inductive loads, as indeed Mr. Esson has indicated.

Coming now to Mr. Esson's paper, I would first like to say that the A.E.G. machines at Moabit, referred to by him, are certainly not the largest constructed up to date in Europe. I shall be pleased to show Mr. Esson, or any other member, machines nearly 20 per cent. larger, and also nearer home. A machine of 3,500 kilowatts has been working at Willesden for six months, and I am now putting down another. These machines are two-phase, and run at 75 revolutions and 10,500 volts.

I will not now discuss the alternators with braced armature frames, referred to by Mr. Esson (and also by Mr. Scott), as I did so in considerable detail, along with many other matters connected with alternator design referred to by both authors, in a paper published in *Engineering* last June; I would, however, like to make a few remarks on certain other questions raised by Mr. Esson.

I would like, for one thing, to corroborate what Mr. Esson says with regard to "core" transformers; it is indeed a matter of surprise that transformers of any other type are nowadays put down. Not only is the cooling very much better, as a whole, as Mr. Esson says, but no part of the windings gets very hot—with a shell transformer, although the outside parts may be cool, certain internal parts are unavoidably far hotter than is safe, and breakdowns of the high-pressure coils are common from this cause, Then again, a core transformer can be built with butted joints, which I consider an advantage for power work (here the higher no-load current does not matter) because of the ease with which repairs can be executed. Finally, owing to the small winding depth, and to the high reluctance of the leakage paths, magnetic leakage can be reduced to a very small amount, which results in a very small pressure drop, even on inductive loads, as Mr. Esson states.

I am rather surprised Mr. Esson has not referred to the question of cooling in connection with transformers. With large transformers, it is the most difficult matter the designer has to tackle, and it may be said that in order to avoid an excessive amount of material, and an

Eborall. impaired electrical performance, some form of artificial cooling is necessary with sizes above about 100 kilowatts, while it may often be used with advantage even earlier. There is no time now to discuss the rival merits of oil and forced draught cooling in detail, but I may say that I consider forced draught cooling preferable on the whole, provided the transformers are in a dry place, principally because it is cleaner and more convenient.[1] With very large oil-cooled transformers the oil has to be circulated, or water cooled, and such large amounts of oil in, for instance, a sub-station, are not very nice; again, once the windings of the transformer have been immersed for some time in the oil, it is not advisable to touch them again, as the insulation generally suffers. If a coil in such a transformer burns out, it often means that the whole winding has to be renewed for this reason.

Referring to the compensated motor invented by Mr. Heyland, referred to by Mr. Esson, I would say that the author has made a slip here, as it is a pure induction motor and nothing else. It is simply a single or polyphase induction motor, fitted with a small and and absolutely sparkless commutator, in addition to the slip rings, and differs from the standard motor only in the fact that its power-factor is practically unity at all loads. It works with a rotating field and a short-circuited rotor, just as usual, the only function of the commutator gear being to supply the compensating wattless currents; the speed is synchronous at no load and slips a few per cent. with the load, as usual. I do not think such motors will ever be much used in small sizes, seeing that the cheaper and simpler standard motors have already power-factors of 80–90 per cent., but for large motors, particularly for slow-speed motors with very many poles, the Heyland compensating device is undoubtedly of sound commercial advantage.

Finally, I would like to refer to Mr. Esson's statements relative to the question of the pressure regulation of polyphase alternators. From Mr. Esson's remarks, it might be inferred that he and other designers could, if they liked, actually build a standard line of generators having a drop of only 2½ per cent. on a more or less inductive load, but that this is not the case everybody knows, as it is nearly a physical impossibility to build commercial iron-cored machines with such drops.[2]

Of course, if the alternator is tested upon a load-possessing capacity, such a small full-load drop could be obtained, but not otherwise, and neither Mr. Esson nor any one else can show us examples of commercial standard machines with actual drops of this order. This being so, (and there is no doubt about it whatever), I entirely fail to see the force of Mr. Esson's remarks on the subject of the relative weights of British and Continental polyphase generators.

I must here confess that the knowledge I have of the weights of standard English polyphase generators has been obtained by looking

[1] For pressures above about 15,000 volts, however, an oil-cooled transformer must, as a rule, be employed, on account of other considerations; that is to say, because of the valuable properties of the oil from the insulation point of view. The above remarks are only intended to apply to transformers which have to be used at the more usual pressures of 10,000 volts and less.

[2] I do not of course refer to turbo-alternators and other special types in this connection, but simply to standard machines.

at them, for, contrary to the universal practice on the Continent, not one British maker publishes the weights of his polyphase apparatus. On the other hand, I know from experience that the machinery of first-class Continental firms is quite heavy enough (leaving out such undesirable constructions as those of the braced frame generators described by both authors), and that the performance and construction of polyphase plant, as built by Brown and similar firms, leaves nothing to be desired in the present state of the art. If British machines are, or have been, too heavy (and I doubt this very much) I attribute it to the fact, and I think most reasonable people will agree with me, that the experience of this country in such work is practically nil, compared with that of the Continent. Mr Eborall.

As already indicated, Mr. Esson's drop of $2\frac{1}{2}$ per cent. is an impossibility with a standard iron-cored modern alternator. The best that can be got with such a machine is about 4 per cent. upon a non-inductive load, and 12 per cent. upon a load of 80 per cent. power-factor. But, as Mr. Esson quite rightly says, the Continental machine usually has a 5–6 per cent. and 16–18 per cent. drop under these conditions respectively, and it becomes necessary to see why this is.

I must first point out that Mr. Esson is wrong in saying that small drops of the order he mentions are unnecessary in practice, for nothing would be more desirable if they could be obtained. The successful working of any three-phase lighting and power system depends to a very large extent upon the quality of the pressure regulation—if the generators have large drops the lighting cannot be otherwise than bad, while the performance of the motors will be affected, for the reason given by Mr. Scott in his paper. Again, for three-phase railway work, good pressure regulation of the generators is an absolute necessity, as the following example will show. On a railway with but few trains running, it may quite well happen that the load on the generators varies within a few minutes from zero to full load. At this latter load, the pressure on the terminals of the motors will be normal, and the pressure drops on the system would be about as follows :—

Trolley lines...	5 per cent.	Expressed in terms of the 'bus-bar pressure.
Transformers	4 per cent.	
Feeders	5 per cent.	
Generators	16 per cent.	
Due to engine governors ...	5 per cent.	
Total	35 per cent.	

So that, for instance, if the full load 'bus-bar pressure is 6,000 volts, hen this pressure would be constantly and quickly fluctuating between bout 6,000 and 8,000 volts, while that on the cars would vary between oo and 650 volts, for instance, which Mr. Esson will agree with me is ot at all desirable. As a matter of fact, the variation might be even eater than this, necessitating automatic regulators to help take care it, but the smaller the generator pressure drop, the better will such gulators work.

Close pressure regulation of the generators is therefore highly

Eborall. desirable, and of the utmost importance. Why, then, are not standard machines designed to give the best possible results as indicated above? Simply because, by doing so, we lay ourselves open to fresh troubles, in the way of parallel running, for with slow-speed engine-driven generators, drops of less than about 15 per cent. are inadmissible except in special cases. The requirements of proper parallel running govern the permissible pressure drop, and nothing else, as several people have found out to their cost.

This brings me to the last point I wish to raise—it is surprising that neither Mr. Esson nor Mr. Scott have made any reference to the compounding of alternators, which is perhaps the most important and most interesting subject engaging the attention of up-to-date designers at the present time. Unfortunately, there is no time to go into this matter now, and hence I would only say that, in my opinion, the most promising solution of this problem is most likely to be found in the employment of asynchronous generators instead of synchronous machines. The difficulties in the way of effectively compounding the latter, not only for varying currents, but also for varying power-factors, are very great, partly because the standard alternator is essentially a machine having relatively high armature reactions. Again, even if a simple method is eventually found, a little reflection will show that difficulties will undoubtedly arise with the parallel running of several compound machines—a totally new set of conditions is brought about, and it is difficult to see how they would ever be overcome.

On the other hand, the asynchronous generator (that is, the induction motor run above synchronism) is a machine which operates by reason of its reactions, and hence the problem is greatly simplified—moreover, as it is asynchronous, it runs perfectly in parallel under all conditions; the operation of such machines is as simple as, and entirely analogous to, that of shunt-wound direct-current dynamos.

And I would like to draw your attention to the fact, that the great value of the Heyland compensating device is in connection with such asynchronous generators, for with its use the machines are made self-exciting, and with a simple addition, they can be compounded or even over-compounded. To give you an idea as to what such machines will do (they are now on the market), I may say that a compounded asynchronous generator of 200 kilowatts, has a pressure drop of under 3 per cent. at full load, no matter what the power-factor of this load may be. There is, of course, no exciter, and the little commutator taking the place of the latter is only 14 inches in diameter, and 6 inches in width, the number of segments being about 100 for a 20-pole machine. This commutator works as smoothly and sparklessly as a slip ring would—in fact sparking cannot arise.

I would like to say much more about these interesting machines and their application, but this is out of the question now. I will merely say, in conclusion, that if our President will allow it, I should be pleased to bring down a small machine and show it to you—perhaps working—one night after a meeting.

Barker. Mr. J. H. BARKER: Mr. Esson, in his paper, has not referred to what is, and must be, the dynamo of the future, the real high-speed dynamo,

running at any revolutions from 1,000 to 3,000 per minute. The fact that one of 3,000 H.P. can be supplied weighing, combined with turbine, 85 tons, against the ordinary low-speed reciprocator, with dynamo of 400 tons, is enough, of itself, to tempt all builders to embark on the turbine dynamo. Mr. Esson says that as the periodicity in his conductors goes up, the design of a direct-current dynamo increases in difficulty. He fixes the English periodicity at something like 15. The problem that turbine dynamo builders have to face is anything up to 60, and in this country it has been attempted single-handed. It is to be hoped that dynamo builders will come to the assistance of the single-handed, and give the benefit of their experience and brains in turning out a continuous-current dynamo running at this high speed. It is not to the credit of this nation that the large size turbine machines are almost exclusively confined to our Continental and our American neighbours, and it is a subject of grief that the new alternators for the District and Metropolitan railways are not being built by English makers, but by Americans. Mr. Barker.

It is almost essential now to adopt cast iron for machines where delivery is promised in four months, with a wait of two and sometimes three months for the delivery of cast steel. Cast iron for the dynamo can usually be made on the premises; it would be well if consulting engineers allowed the supply of cast iron in practically all cases. With turbine dynamos the magnet casting is an essential part of the whole machine, and it is absolutely necessary that this part should be almost the first to be used. At present the economic limit for high-speed dynamos for continuous current is about 250 kilowatts. They can be made perfectly satisfactory and to give economical regulation, whilst for continuous running there is no machine which can compare with it.

It is gratifying to hear a man of Mr. Esson's experience approving of the tandem dynamo. By putting one or more dynamos in tandem the speed can be maintained and a highly economical machine as regards steam consumption easily obtained. However, engineers, if shown a tandem machine, will at once rule it out of court.

The drop of alternators has been referred to by the previous speaker, as to which, he says, it is practically impossible to get a lower drop than 4 per cent. In the design of a large alternator as low as $1\frac{1}{2}$ per cent. has been obtained without a very extravagant expenditure of copper in the magnet coils. Such a drop, for practical purposes, is ridiculously low. Recent inquiries show that engineers admit drops even as high as 7 per cent. But if low drops are required, they can be obtained in a high-speed alternator very much more cheaply than in one of low-speed multipolar.

Structural details have been referred to at some length, and it is interesting to hear the experience of slow-speed dynamo builders. The objection to large plates for armatures, 4 feet and 5 feet, have been referred to. But plates over 4 feet cannot be obtained of a much less thickness than 20 mils. For good design for these high-speed machines it is almost essential that there should be nothing more than about 14 mils. The large plates vary in thickness to an appreciable degree. The centre of the plate is tight up and the outside slack, with the usual

Mr. Barker. detrimental results. Plates undoubtedly should be punched and not milled. Punching machines can be bought to-day which will give satisfactory results, and the core built up so true that they need not even be touched with a file, and tubes put through without any trouble.

For insulation there can be no question that paper is much better than any form of varnish or other insulation. The microscopic vibration with a dynamo, and particularly turbo-dynamos, will disintegrate varnish pretty quickly. Paper will last for many years, and in some armatures after continuous service the paper has been practically as good as when it was first put in. As to press-spahn, I have personally had experience of this up to 6,000 volts continuous working with a test of about one and a half times that. It is preferable to micanite or almost any other form. The building up of the tube is in your own hands. More care can be assured that the material is free from any conducting substance, and a very much better article to hand. For drying the author does not object to a temperature of 250° Fahr. It is very objectionable to subject any armatures to such a temperature. A manufacturer of cotton-covered wire, who had carried out exhaustive tests, states that at 70° C. the tensile strength of cotton begins to deteriorate very rapidly. In fact in vacuo drying can be done at a low temperature, and the result is much more satisfactory. The apparatus is not costly; a few pounds will buy a disused boiler which, connected to the works condenser, having half a dozen coils of pipe carrying live steam, gives all that is required.

In conclusion, I would urge on the British manufacturer the necessity for building these high-speed dynamos. It is a pity that more attention has not been turned to them. Representing a firm of turbine builders, I would offer all possible assistance in supplying such engines apart from the dynamos.

Mr. Hawkins. Mr. C. C. HAWKINS: In spite of the fact that the "output-coefficient" in any given line of dynamos follows a regular law, as mentioned by Mr. Eborall, yet I think one must agree with Mr. Esson that on this point there is a surprising divergence between the machines of different makers. M. Rothert, in *L'Éclairage Électrique,* published a careful analysis of most of the alternators at the Paris Exhibition, and found that even in machines of the same order of size this coefficient varied in the proportion of one to four; when two machines, both by good makers and of somewhat the same size, were compared on the basis of one pound of copper being the equivalent in cost of five pounds of iron, the total cost of the active material in the one machine was five times that of the other. This is borne out by Mr. Scott's Table VII., which is mainly derived from the alternators of the Paris Exhibition. The highest coefficient is that of No. 3; but, putting this on one side, owing to its low frequency of 25 as compared with 50 periods in the other machines, the lowest and highest are Nos. 10 and 8, the latter being apparently two and three-quarter times as good from the manufacturers' point of view. There is perhaps rather less divergence in continuous-current machinery, yet one sometimes comes across a machine of exceptionally small size, which no purchaser could question, whether on the score of temperature-rise, of sparkless commutation, or of sound construction.

Such divergencies between the machines of firms of equally good reputation, after discounting any great difference of speed or voltage or frequency, can only be traced in the long run to lack of experience or of designing ability, and more often to the former. What we really want is not so much average figures, which are fairly represented by Mr. Scott's tables, but the maxima that can be attained to serve as ideals. One machine which has been seen and proved to be thoroughly good is worth more to the observer than many tables of average results. Mr. Hawkins.

Turning to one or two details in Mr. Scott's paper, the roller machine of Fig. 16 was tried by the firm with which I am connected in 1897, but was soon given up, as it was found that it was not so expeditious as the mallet. In the hammering process, when carefully done, there is little danger of damage to the insulation, and in a few moments the bend is imparted to the bar exactly as required, especially at the inner corners of the outer ends.

Mr. Scott says that "it does not appear to have occurred to any one that it is quite unnecessary to bend both ends of each conductor." I think that it has occurred to many people, but the single bend has the obvious disadvantage that the axial length of the end-connectors is then double that of the lozenge-shaped coil, and this is especially disadvantageous if the poles are few in number.

The series-parallel winding of Prof. Arnold has the advantage, as is well known, that the designer can choose any even number of armature paths (two or more) independently of the number of poles. Yet it has always appeared to me that it must labour under the disadvantage that if more than two sets of brushes are employed, there is no automatic corrective to ensure an equal division of the current between the different sets of brushes of the same sign. The current may, in fact, shift from one set of brushes to another and back again, according to the small differences in their contact-resistance which they may offer at any moment. The winding is certainly largely used on the Continent, and I should like to ask any gentleman who has experience of it whether the objection which I have mentioned is in practice really to be feared.

With regard to carbon brush-holders (p. 387), the wording of Mr. Scott's first condition for a successful type would appear to condemn the slider holder as inferior to the hinged-arm holder. If the carbon block is to be fixed at the end of an arm, certainly it should be *firmly* fixed, and the arm should be as long as possible. My own observation has however led me to think that the slider type has a slight superiority due to the fact that the brush is only tossed up and down and not thrown off the commutator surface so as to describe an arc, as it passes over the slight inequalities, almost imperceptible, which must exist between the segments even of the best commutator. Further if Mr. Scott's second condition—that the inertia of the moving part should be reduced as much as possible—is of great importance, which I am rather inclined to question, it is best obtained by making the light carbon block the only portion which moves.

Finally, what has been the experience of Mr. Scott with regard to the vacuum *apparatus* in the practical drying, not so much of simple

rkins. coils, as of finished armatures, especially after they have been newly painted or varnished? While the vacuum will quickly extract say 95 or perhaps 99 per cent., is there not considerable difficulty in extracting the last one per cent., which really keeps the insulation resistance still low, and is not the process longer on that score than in the old-fashioned stove?

Sparks. Mr. C. P. SPARKS: On page 331 of Mr. Esson's paper he contrasts single and polyphase alternators as follows:—

> "The drift of practice has been towards making an alternator in all respects satisfactory from an engineering point of view. It must be first and foremost a machine, and a machine that will run continuously without giving trouble."

I disagree with Mr. Esson's views that the single-phase alternator has been abandoned through being unmechanical. Three satisfactory types were developed in this country: the Ferranti, the Siemens, and the Mordey. The reason that single-phase machines are not so largely used as heretofore is due to the change in methods of distribution, owing to the flexibility of continuous current in meeting the varying demands of consumers. This system of supply is now adopted in nearly all large towns, the distribution of continuous current having been materially aided by raising the pressure of supply of three-wire systems to 250/500 volts. In some instances where a town started to supply single-phase alternating current, the system of distribution has been changed to polyphase. In the majority of towns the change has been from single-phase alternating to continuous current, and in cases where the stations are outside the area of supply, polyphase currents are used for transmission to the substations.

Owing to single-phase alternators being unsuited for transmission schemes, we have lost one great advantage possessed by this type of alternator, namely, a comparatively definite wave form. Although polyphase alternators are by degrees being made to give approximately a sine wave, there is great difficulty in obtaining a machine giving as true a wave form as that obtained from all single-phase machines with copper-tape armatures.

Mr. Esson, under the heading of "Conversion," puts before us the relative merits of synchronous and asynchronous motors. In starting a new transmission scheme, either rotaries, asynchronous, or synchronous motor generators can be used for conversion. If the whole energy is transmitted to substations for conversion to direct current, a low frequency (25) will be chosen and rotaries used, but in combined schemes where a certain amount of lighting and power supply is to be given by alternating currents in addition to the supply of continuous current, a higher frequency (50) will be chosen. This frequency cuts out the rotary. As to the relative merits of synchronous and asynchronous motors, if you are supplying alternating current for lighting, as is specially the case in schemes where single-phase systems are being converted, you are bound to use a synchronous motor generator. Mr. Esson himself pointed out during the discussion of Mr. Eborall's paper, the difficulties that arise in controlling the pressure on the H.P. feeders when starting up asynchronous motors. By using synchronous

motors started from the direct-current side you have the minimum interference with the pressure of the lighting system. The synchronous motor generator has a further advantage over the asynchronous, as its speed is independent of small changes in pressure, being governed by the generator speed, whereas the asynchronous motor generator is dependent both on speed and pressure. Mr. Sparks.

The PRESIDENT: Mr. Kilburn Scott has told me that he would prefer not to speak his remarks, but to write them in the *Journal*, as it is getting late. The President.

I propose a hearty vote of thanks to Mr. Esson and to Mr. Kilburn Scott for their papers. The discussion has been exceedingly good. It has not been what you call a lively discussion, but it has been an exceedingly technical discussion, and very valuable, especially to those who are particularly interested in dynamos.

I will, therefore, put it that we carry a hearty vote of thanks to the authors of the papers.

The motion was put and carried unanimously.

Prof. SILVANUS P. THOMPSON, F.R.S. (*communicated*): Many questions of interest are raised by the papers of Mr. Esson and Mr. Scott. These remarks will deal with only a few of them. Professor Silvanus Thompson.

The relation of dimensions to output have often been discussed. The numeric called by Mr. Esson "output-coefficient" and by Mr. Scott "size constant," viz., the product of revolutions per minute by square of diameter and by length of core divided by output (watts), does not appear to be a very satisfactory quantity. For continuous-current machines Mr. Scott gives in Table III. values varying from 0·0013 to 0·0345, so that so far from being a constant it varies enormously. For alternators Mr. Esson gives 0·015, while Mr. Scott in Table VII. shows actual values varying from 0·0118 to 0·053. For continuous-current machines the highest value is 26 times the lowest; while for alternating machines the highest value is about 4·6 times the lowest.

I have found as a much more useful guide to preliminary design the Steinmetz coefficient, namely, the product of diameter and length of core divided by the kilowatts. For the same machines (omitting the first in Table II., which is known to be overrated) the values run from 2·0 to 6·3 for continuous-current machines, where the highest value is only 3·15 times (not 26) the lowest. For alternators Table VII. shows that the values run from 1·4 (for a machine the output of which is also probably overrated), or 1·66 to 4·2.

The "output-coefficient" is based on the assumption that the output of a core is proportional to its volume. The "Steinmetz coefficient" proceeds on the view that the output is proportional to the surface. Mr. Esson makes the remark on p. 338 that "temperature is the factor which limits the output." This, for a given efficiency, is unquestionably true; and as temperature rise depends upon the available surface for getting rid of the heat, the output (for machines of given efficiency) is manifestly determined by the surface and not by the volume of the core; hence the $d \times l$ of the "Steinmetz coefficient" is a truer measure than the $d^2 l$ of the "output-coefficient," in spite of

fessor anus mpson. the fact that the former does not take account of differences of speed. In fact, if equal magnetic densities and current densities were observed in the "active belt," the Steinmetz coefficient would simply vary inversely as the surface-speed. Or if surface speeds were all equal, the higher the specific utilisation of iron and copper in the "active belt," the lower would the Steinmetz coefficient be. Mr. Esson's remark on p. 338 that "the so-called active belt becomes more active as the speed is reduced" is misleading, because it refers to engine-speed, not to surface-speed; many of the dynamos which he puts down as slow-speed having, by reason of their large diameter, really high surface-speeds. It is the surface-speed, not the revolutions per minute, which should be considered in comparing one diagram with another.

The reason why $d^2 l$ is not proportional to the kilowatts per revolution, as appears from the figures given by Mr. Scott, is that small machines and large are not worked with the same coefficients of flux-density and of ampere-density at the periphery as large ones: and they cannot be, because of the conditions of ventilation and sparking being different. The proportion is also affected in a way not generally recognised, by the ratio of the armature-surface covered by the pole-shoes to the total peripheral-surface of the armature. Let this ratio, which is the same as the ratio of pole-span to pole-pitch, be called ψ. Then the flux from one pole may be represented by the equation—

$$\begin{aligned} N &= B_g \times \text{pole-face area}, \\ &= B_g \times \psi \times \pi \times d \times l \div p\,; \end{aligned}$$

where B_g is the mean flux-density under the pole-face, d the diameter of the armature, p the number of poles, and l the length of core parallel to shaft, taken as equal to the axial length of the pole-shoe. Further, the ampere-density per inch along the periphery of the armature, which may be denoted by q_1, if divided by the number of amperes per conductor and multiplied by the periphery in inches, gives the total number of armature conductors Z; or if we write C for the whole armature-current, and c for the number of circuits through the armature, we have—

$$Z = q_1 \times \pi \times d \times c \div C.$$

Now the general formula for continuous-current machines is—

$$E = \frac{p}{c} \times \frac{\text{R.P.M.}}{60} \times Z \times N \div 10^8,$$

and inserting the above values of Z an N and writing $E\,C \div 1000 =$ K*w*, we get—

$$d^2 l = \frac{59{\cdot}2 \times 10^{10}}{B_g \times q_1 \times \psi} \times \frac{Kw}{\text{R.P.M.}}.$$

This formula shows at once why the "output-coefficient," as Mr. Esson calls it, is not constant. If we inquire as to the value of the quantities, we find that in modern machines it is not expedient (for commutation reasons) to make B_g less than 40,000 lines per square inch, nor (for distortion and heating reasons) to make q_1 more than 600

amperes per inch run, while ψ is preferably about 0·75. Putting in these figures, we get for the numerical coefficient— Professor Silvanus Thompson.

$$d^2 l = 33{,}000 \times Kw \div R.P.M.$$

This corresponds to a value of 0·03 in the coefficients as used by Mr. E. K. Scott.

If we make the further assumption that for cast-steel poles of cylindrical shape, the diameter may be taken equal to l and to half the pole-pitch, with the relation $l = d\,\pi \div 2\,p$, then the substitution of this value gives the solution—

$$d = 7{,}222 \times \sqrt[3]{\frac{Kw \times p}{R.P.M. \times B_g \times q_1 \times \psi}}.$$

This formula gives excellent figures for machines of this class. Or, if in order to use square poles for laminated pole-cores we make $l = d \times \pi \times \psi \div p$, we shall have—

$$d = 5{,}737 \times \sqrt[3]{\frac{Kw \times p}{R.P.M. \times B_g \times q_1 \times \psi^2}}.$$

Both the authors comment on the practice adopted by some firms of introducing a resistance of german-silver or nickeline into the commutator risers, a practice supposed to promote sparkless commutation. But neither of them expresses any opinion upon this device, which the present writer believes to be founded on a fallacy. Some makers are equally anxious to avoid any such unnecessary resistance, and are able to produce excellent machines without it. Discussing this point recently with the late Prof. Short, I found him to agree with me.

I have recently expressed my ideas about the design of continuous-current machines in my book on *Design of Dynamos*, and therefore may pass on to practical points. Mr. Scott mentions several modes of insulating core-discs, but does not refer to one that has given satisfaction, namely enamelling them with a wash of water-glass. This is far superior to japan, which is liable to be thrown out when the machine is heated after a long run. I doubt whether sal-ammoniac possesses the property of dissolving iron burrs, which he attributes to it on p. 371. Further, the corroding of copper found to take place in coils treated with shellac varnish must be attributed to the vegetable acids in the lac, not to the alcohol in which it is dissolved.

Mr. Scott refers to the plan (originated by Dobrowolsky) of fitting a thin iron pole-ring or liner around the armature to gradate the magnetic field. The latest improvement on this is a device due to Mr. Murray, not mentioned by either author, of adapting to the pole-cores a ring carrying laminated pole-pieces, in which the stampings are specially disposed so as to secure the proper disposition of field with high saturation at the entrant pole-tip. Messrs. T. Parker & Co. have found great satisfaction with this device.

Mr. Scott's airy disposition of the tooth and slot question on p. 374, by saying that "the simplest way to set out the width of slot and tooth is to make them about the same width at the periphery," will not suffice for the needs of modern good design. A much more detailed consideration is necessary. The recent book of Dr. M. Corsepius shows

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how important this part of dynamo design is—and it runs through all types, alternators and motors as well as dynamos. Mr. Scott attributes the barrel-type of armature windings to Mr. Parsons: but it can hardly have been said to be a success until the two-layer winding was produced by C. E. L. Brown. Mr. Brown also ought to be credited with the device shown in Fig. 17, which he introduced six or eight years ago. It exists in the alternators at the works of M. Baly.

The use of equalising rings as an adjunct to multipolar windings, mentioned by Mr. Scott on p. 379, does not appear to be appreciated by British engineers. In every armature with parallel windings there is, necessarily, if the field-poles are not all of equal strength (and they are not of equal strength if the air-gaps are not alike, even though the cores and exciting ampere-turns are all alike), an inequality in the voltages which they induce, and therefore in the currents they generate. Any inequality in the currents generated in the parallel paths produces two results:—(1) The total resistance of the armature rises; (2) the armature-currents tend so to react as to strive to equalise the fields. The increase of total resistance owing to the unequal distribution of current may seriously affect the heating of the machine, and constitutes one of those obscure causes of waste often vaguely attributed to eddy-currents. Although the point is fairly obvious, it is worth an additional word. Suppose two conductors of 1 ohm each are in parallel. One is apt to suppose that the total resistance of the two in parallel is $\frac{1}{2}$ ohm. So it would be if the whole current divides itself equally between the two. For example, 200 amperes dividing itself equally between them will waste 10,000 watts in each conductor, or 20,000 watts in all; being therefore equal to $(200)^2 \times \frac{1}{2}$. But if for any reason the current were to divide itself unequally, say into 120 and 80 amperes, the heat waste would be $14{,}400 + 6{,}400$, or in total 20,800 watts; the resistance of the two paths in parallel being not $\frac{1}{2}$ ohm but 0·52 ohm, since $(200)^2 \times 0{\cdot}52 = 20{,}800$. Even so in a parallel-wound armature, the more unequally the current divides itself, the greater is the total resistance offered by the windings. Further, if the current divides itself unequally there will be an undue amount of current to collect at some one or more of the sets of bushes, giving rise to spark troubles. It is mainly to avoid this that in multipolar armatures equalising connections have been found advantageous. They enable the equalising currents of reaction to circulate with a minimum of disturbance to the collection of current at the brushes. This is also the action of the closed-circuit windings devised by Mr. B. G. Lamme for armatures having a series-parallel winding; but these closed windings are quite independent of the ordinary winding, and are not connected in any way to the commutator. With respect to field-magnet windings, it may be pointed out that the "old dodge in telegraphic instrument making" mentioned by Mr. Scott on p. 394 will certainly not effect any reduction of the self-induction of the coils.

Turning to alternators, it may be observed that while Mr. Esson on p. 352 declares high saturation of the field-magnets necessary to good design, Mr. Scott on p. 409 recommends that the pole-cores should have ample area so that the magnetic flux-density may be kept as low as

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possible. In this divergence of view I unhesitatingly take sides with Mr. Esson. Unless the pole-cores are saturated up to, say, from 95,000 to 115,000 lines per square inch, the machine will have a disastrous drop on an inductive load; and the ampere-turns spent on the pole-cores instead of being (as in most continuous-current machines) a negligible quantity compared with those spent on the air-gap, ought to be from 20 to 35 per cent. of the entire excitation. Both Mr. Esson and Mr. Scott assume that the magnet-wheel of a modern alternator may serve as the engine flywheel. This is, to my mind, by no means proven for all cases. The design of flywheel suitable for a given engine may be by no means suitable for the alternator it is to drive; for the most fundamental point in settling the design of an alternator is the frequency of the currents which it is to give. On this depends the number of poles; and as the poles cannot be either enlarged or diminished in their pitch outside certain well-defined limits, the diameter of the alternator is fixed by considerations quite other than those which determine its suitability as a flywheel. With so high a frequency as 50 periods per second it is often difficult to give the magnet-wheel a sufficient moment of inertia for flywheel purposes without making it enormously heavier than the flywheel which an engineer would have designed for the same engine. Mr. Esson is quite right in saying that engineers (and not British engineers alone) have been putting too much material into their alternators. I have in my mind two alternators at the Paris Exhibition for the same output at the same speed, one of which had eight times as much iron (magnetic iron, not including mere construction work) and eight times as much copper as the other. A most exhaustive criticism of the Paris alternators was published at the time by Mr. Rothert, in which this and many other striking facts were brought out. One most distinct fact is not alluded to either by Mr. Rothert in his report, or in either of the two present papers, namely, that in modern alternators having the most diverse properties as to voltage, speed, efficiency, drop, and specific utilisation of material, one thing remains almost constant throughout—namely, the the pole-pitch. And the pole-pitch is almost invariably about 10 inches at the working face. Of the 14 machines in Table VII. of Mr. Scott, all were for a frequency of 50 ∿ save one at 48 ∿, and one at 25 ∿. Their pole-pitches are as follows:—11·8; 10·02; 9·3; 9·4; 9·75; 10·6; 9·3; 9·5; 9·5; 10·6; 11; 9·8; 9·7; 8·3. Leaving out the first (a 3,000 kilowatt) and the last (a 300 kilowatt machine), they all lie between 11 and 9·3 inches, with an average of 9·75. The figure is far more of a constant than any "output-coefficient." In fact, to begin the design of an alternator, the safest way to fix its size is to ascertain from the prescribed frequency and the engine-speed the number of poles, multiply this by 10, and one has the circumference of the working face (in inches). This shows also why, in the desire to have designs that can be built with pole-cores of circular section, the core-length from front to back is so seldom less than 8 or more than 13 inches. Mr. Scott suggests that the length tends for large sizes to remain at about 10 per cent. of the diameter: in which suggestion I do not agree. Mr. Scott refers to the superior strength of magnet-

fessor inus mpson. wheels built up of two webs of rolled-steel plate instead of having cast-iron arms, and instances the 5,000 k.w. alternators of the Westinghouse Company erected in 1902. An instance nearer home is presented by the alternators built by the late Mr. Gordon for Paddington just twenty years previously. Mr. Esson suggests, on p. 347, that the system of clamping rings and bolts that hold together the armature-core will, if not insulated, form an *amortisseur.* If they do, all that I can say is: so much worse for the machine; for the amortisseur will be entirely in the wrong place. It is wanted on the magnet system to steady the magnetism against pulsations. It is simply harmful on the armature if it acts at all to prevent pulsations of the magnetism at the prescribed frequency.

Each of these papers merits our study; and they mark the recent development of the subject. Mr. Esson's is particularly valuable for its contrasts and criticisms; Mr. Scott's for its numerous practical details, and not least for its references to modern workshop appliances for use in dynamo construction.

essor ertson. Prof. DAVID ROBERTSON (*communicated*): In Mr. Scott's otherwise most excellent paper the remarks on armature windings (p. 379) are almost entirely erroneous. For a "Parallel Grouping," *i.e.*, a winding with as many circuits as poles, the number of slots and commutator bars need not be even; and for a "Series Grouping," or a winding with only two circuits, they need not always be odd. For the latter winding they may be either odd or even when the number of pairs of poles is odd, although they must be odd when there is an even number of pairs of poles. Similarly, the "Arnold Series Parallel Winding" may have an odd number of segments when the number of pairs of poles is odd. Again, with any practical example of the last winding, which is a singly re-entrant wave winding with as many circuits as poles, if we start at a positive brush and go through as many conductors as there are poles (*i.e.*, nearly once round, or through *four* of the heavy loops in Fig. 18), we do not come back to a segment near to the next negative brush, but to one close to the one we started from, and distant from it as many segments as there are pairs of poles. True, in the example given in the paper we do come exactly to the next negative brush, but this is only because of the small number of segments there chosen. The number of segments (4 for the 8-pole machine) between the start and finish of an incomplete round is the same whether the actual number of segments per pole is 4 or 40, and consequently with the numbers used in practice we return much closer to the brush we start from than to the other. All symmetrical windings, including the "plain series winding," may have as many sets of brushes as poles, and all wave windings may be run with any smaller number, down to two, if desired, provided the brushes be of sufficient width and the segments per pole not too few. Lap windings, on the other hand, must have as many brushes as poles, unless the commutator segments are cross-connected.

With wave windings the conductors are short-circuited through the connections between the several brushes of the same sign, and also, several in series, through the tips of the brushes when these are wide ugh. When only two brushes are employed, they must be at least a

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certain width to allow any short-circuits to take place at all. With 8 circuits, as in Fig. 18, the brushes would have to be more than three segments in width if only two are to be employed. With the small number of segments there shown this would short-circuit the whole machine, and would therefore be impracticable, but with the much larger number that would be employed in an actual machine it would be quite feasible. The circuits are more symmetrical when all the brushes are employed, but owing to the way in which the short-circuited coils come in between the several brushes of one sign, the current will not divide equally between them, and its distribution will fluctuate with the phase of commutation. Hence a greater total brush area is required than would be necessary if an equal distribution could be secured. On the other hand, the number of conductors cut out of short-circuit at once is only two with the maximum number of brushes, whereas with only two brushes it would be equal to the number of poles. Better commutation may therefore be expected from the large number of brushes in spite of the greater care required in adjusting them.

In the lap winding, where the several brushes of one sign are not adjacent to one another in the winding, and where the conductors forming one circuit are confined to two adjacent poles, the armature reactions assist in ensuring the equality of the currents in the different circuits and at the different sets of brushes. Against this must be set the fact that each circuit of a wave winding has approximately an equal number of conductors at every pole, and that therefore the balance of the circuits is disturbed much less by inequalities in the polar strengths than in a lap winding. Mr. Scott seems to use "parallel" and "series" to distinguish between lap windings, in which the front and back pitches have opposite signs, and wave windings, in which both have the same sign. The latter terms, lap and wave, are preferable when denoting the mode of winding, whereas the electrical properties are better indicated by the total number of circuits and re-entrances than by the names "parallel" and "series," which do not include all possible cases, and are not used in this connection with a very definite meaning. Thus, the "Arnold Series Parallel Winding" of Fig. 18 would be called an 8-circuit, singly re-entrant, 8-pole, wave-wound drum; an ordinary "Parallel Grouping" for the same field an 8-circuit, singly re-entrant, 8-pole, lap-wound drum; and an ordinary "Series Grouping" a 2-circuit, singly re-entrant, 8-pole, wave-wound drum. The use of the terms "Parallel Grouping" and "Series Grouping" is responsible for the common, but erroneous, notion that these are the only possible arrangements, at least with singly re-entrant windings. It is possible to design a winding to give any even number of circuits whatever with any given even number of poles.

To take only the simplest case, viz., that in which the cycle of the winding is repeated after going through two conductors, or groups of conductors, and their end connections, let [1]

[1] For a proof of these formulæ, and a fuller discussion of the conditions involved, see Robertson on "A General Formula for Regular Armature Windings," *Journ. Inst. E. E.*, vol. 31, p. 933.

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G = Number of conductors, or groups of conductors when a group is treated as the unit.

p = Number of poles.

c = Total number of circuits through armature.

w = Number of separate, but identical, windings, each having c/w circuits.

y_f, y_b = Front and back pitches of winding.

$\bar{y} = \frac{1}{2}(y_f + y_b)$ = Average pitch of winding = Number of times you must go round armature to trace out complete winding.

m = Number of pole-pitches approximately included in the average winding pitch.

Then all these must be whole numbers, and G, p, and c must be even. For any regular re-entrant winding, in which the cycle of connections is completed by going through two groups of conductors and their end connections,

$$\bar{y} = \frac{m\,\mathrm{G} \pm c}{p}, \text{ or } \pm c = p\bar{y} - m\,\mathrm{G} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\text{and } w = \text{H C F of G}/2 \text{ and } \bar{y} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

For ordinary lap windings m is zero, and for ordinary wave windings m is unity. With many poles higher values are possible for both lap and wave windings, but do not seem to possess any advantages to compensate for the greater amount of copper they would require. We may therefore write :—

$$\text{For lap windings, } \bar{y} = c/p, \text{ or } c = p\bar{y} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\text{For wave windings, } \bar{y} = (\mathrm{G} \pm c)/p, \text{ or } \pm c = p\bar{y} - \mathrm{G} \quad . \quad (4)$$

The individual pitches, y_f and y_b, should be nearly equal to the pole pitch G/p, but it does not matter very greatly what they are so long as they do not differ too much from this, and are not multiples of $2w$. The back pitch is usually taken as the nearest allowable to G/p; other values give chord windings.

Equation (3) shows that for lap windings the number of circuits is independent of the number of conductors, but must be a multiple of the number of poles in order that $\bar{y}$ may be an integer. The number of conductors may therefore be any even number, but it is usual to make it (G) a multiple of the number of poles (p), generally an even multiple, so as to get perfect symmetry of the circuits.

With a wave winding we see from equation (4) that the number of circuits and number of conductors are not independent of one another, but that if c be given, such a value must be chosen for G as will make $\bar{y}$ an integer. This can be done for *any* even value of c whatever. Thus, to get as many circuits as poles ($p = c$), G must be a multiple of p; and for values of c not multiples of p, G must not be a multiple of p.

In going through as many (p) conductors of a wave winding as there are poles, the total travel is $p\bar{y}$, which is equal to G $\pm$ c from (4); c is thus the excess or deficit from being exactly once round expressed in terms of the group space. Expressed in commutator segments, this excess or deficit will be $c/2$ in the ordinary arrangement where there is

one segment for each pair of groups. Hence the number of circuits is twice the number of segments between the start and finish after going through as many groups and end connections as there are poles. Professor Robertson.

Equation (4) may be put in the form—

$$\bar{y} = \frac{G/2 \pm c/2}{p/2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where G/2 is the number of commutator segments in the usual arrangement, and $p/2$ is the number of pairs of poles. The possible numbers of segments mentioned above for the wave windings follow at once from this when it is remembered that $\bar{y}$ must be an integer.

By allowing a slightly greater latitude in the choice of G, we can arrange to get all the (c) circuits in one winding, or have them distributed over several (w) distinct but identical windings, each of which, however, must have an even number of circuits. The winding shown on Fig. 18 has 64 conductors, 8 poles, and 8 circuits. Hence—

$$\bar{y} = \frac{G \pm c}{p} = \frac{64 \pm 8}{8} = 9 \text{ or } 7.$$

The smaller of these values is the one which applies to the diagram. The H C F of $\frac{1}{2}$G and $\bar{y}$ is that of 32 and 7, which is 1. Hence the winding is singly re-entrant. Numbering the conductors at the bottoms of the slots 1, 3, 5, etc., and those at the tops of the same slots 2, 4, 6, etc., respectively, we see that we go across the back from 1 to 10, and across the front from 10 to 15, giving $y_b = 9$, $y_f = 5$, and $\bar{y} = 7$, as above.

But by taking $G = 72$, which is the next highest allowable number and requires four more slots, $\bar{y} = 10$ or 8. The H C F of 72/2 and 10 is 2, giving a doubly re-entrant winding, each separate winding having 4 circuits. The H C F of 72/2 and 8 is 4, and this will therefore give a quadruply re-entrant winding, each of the four components having two circuits.

When the number of circuits required is a multiple of p, the possible values of G form an arithmetical progression whose common difference is p, and the possible numbers of segments another progression with the common difference $\frac{1}{2}p$. But when c is not a multiple of p, the variety is much greater; the common difference of the progression for G is then the H C F of c and p, but those terms which have a greater factor in common with p must be struck out. It thus cannot be said that the limitations to the possible values of G, and consequently of the number of commutator bars and of slots, is a very great objection to wave windings, seeing that the extreme difference between two consecutive possible values only amounts in the worst case to one conductor per pole, or one segment per pair of poles. The chief place where it will be felt will be in limiting the number of coils which can be placed in one slot. Thus in the ordinary tramway motor armature (a 2-circuit, singly re-entrant, 4-pole, wave-wound drum) the number of segments and coils must be odd. Hence we cannot arrange to tape up two coils together, nor four, etc., although we may do so with 3, 5, 7, etc., if we make a suitable *choice of* G. Windings having other numbers of

Professor Robertson.

circuits than two or p do not seem to have been used except with several re-entrances (multiplex windings), and then each separate winding has either two circuits or p circuits. Probably the only reason is that designers are not aware that other arrangements are possible.

The advantage gained by increasing the number of circuits, whether the winding is still singly re-entrant or is made multiplex, is that the amount of current to be commutated at once is reduced. The greatest permissible width of brush is not much altered, because the number of commutator segments must also be increased if any advantage is to be gained by the increased number of circuits. Although the brush spans over more segments, these segments are smaller than before, and there is also a greater amount of space wasted by the mica. There cannot, therefore, be much, if any, room for a saving in the length of the commutator. The gain is in allowing a greater current per pole to be dealt with.

On the matter of commutation (see p. 403) much has been written by many authors. The current which can be collected per pole is limited by two considerations which are not wholly independent. First, we must reverse the current in each section during the time of the short-circuit. This imposes a limit to the product of the current in one section by the inductance of the section. An increase in the number of circuits reduces the first of these factors, and the latter is made as small as possible with the ordinary design by having only one turn per section in all except the very smallest machines. It would, however, be quite possible to exceed the present limits by adding another commutator at the back end of the armature, all the positive brushes on both sides being joined in parallel, and all the negatives. Only one conductor, or half a turn, would then be thrown out of short-circuit at once, with a corresponding diminution of the effect of inductance. The advantage gained by thus doubling the possible number of segments would be considerable with machines dealing with large currents, especially when the brush resistance is the chief factor in effecting commutation rather than the reversing E.M.F. obtained by giving lead. The two short commutators with brushes in parallel would also have considerable mechanical advantages over the single long one of the ordinary design, as the bulging tendency due to centrifugal force is less with the short segments. It would of course cost rather more, but it is worth considering whether it will not pay in machines for very large currents.

The other consideration is that the armature strength should not be so great as to produce too much distortion and weakening of the field. This depends upon the number of conductors per circuit, as well as upon the current per pole, and will therefore depend upon the pressure. It is more important in machines which have no series field coils than in machines with them.

The idea that the armature cross ampere-turns per pole should be less than the field ampere-turns for one gap is based upon the assumptions that a reversing E.M.F. is required, and that the gap is the only reluctance in the path of the armature cross flux. It was originally

deduced for smooth core armatures. In modern machines with saturated teeth and pole-tips we ought also to take account of their reluctance, and the field ampere-turns available for them should be added to those for the air-gap proper. Making these corrections on the table, page 404, the ratio comes to be much more nearly equal to unity. With careful design it is probably quite possible to commutate properly without a reversing E.M.F. in the coils by making proper use of the brush resistance. This permits of a smaller value of the ratio— Professor Robertson.

$$\frac{\text{Gap, etc., ampere-turns per pole}}{\text{Cross ampere-turns per pole}}.$$

The effect of the pole span must also be taken into account. Only that cross magnetising current within the pole span is effective. Any outside that produces little effect. Shaping the poles away at the strengthened horn also reduces the effect of the cross E.M.F., and so reduces still further the limiting value of the above ratio.

On page 366 the author states that shaping away the pole right across is not such good practice as shaping it away from the horn to the centre, "because in the middle of the pole the air-gap should be as short as possible." This is very doubtful indeed, as it seems to be based on the assumption that the armature cross ampere-turns produce no effect at the centre. Although true when the air-gap is symmetrical, this is no longer the case when the poles are shaped away. Owing to the variation of the reluctance of the gap, the position of no effect for the cross turns is shifted towards the side where the gap is shortest, the current in the wires there being more effective than that at the other side.

It may be questioned whether high resistance commutator lugs are of any good whatever. They certainly assist the old current to die out, but they also oppose the growth of the new current in the incoming lug. The correct place for the resistance is at the brush contacts, as there the motion gradually cuts it out of the new path from the brush to the winding and adds it to old path. In fact, as their resistance can only diminish the effect of the variation of contact resistance, high resistance lugs are probably more harmful than otherwise.

With regard to the forming of the armature conductors by bending only one end, mentioned on page 377 and shown in Fig. 17, it is probable that many persons have thought of it, but did not consider that its advantages outweighed its disadvantages. It requires about twice as much space for the end connections than does the usual symmetrical arrangement, and requires a considerably greater length of copper with the corresponding increase of the inductance of each section and diminution of efficiency. The more nearly parallel to the end faces of the core the end connections are made, the shorter they will be for a given span, and the less they will project beyond the core. But the slope is limited by the space required for the conductors, and the limiting slope depends almost entirely upon the ratio of width of conductor, or group of conductors, including insulation and clearance, to the pitch of the slots, and will be the same whether the other half is

Professor Robertson.

straight or bent. The slope must therefore be made twice as long if it has to make the whole span and meet the straight from the other slot, than if the other side is bent over to meet it half-way. With nearly symmetrical end connectors both layers can be equally closely packed together, whereas with one side straight that layer will have a considerable amount of waste space which is not even of much use for ventilation, owing to the closeness of the sloping layer. The straight side certainly has considerable advantages in the matter of threading into tunnels and insulation, but it is a matter for consideration in any particular case whether these are sufficient to compensate for the increased dimensions and weight involved. Fig. 17 seems to show a coil short-circuited on itself. This, of course, is a slip.

The note on page 375 is not quite clear. Under what conditions does the flux distribution curve assume the form there mentioned?

How can the "dodge" of winding all the layers of a coil in the same direction, mentioned on page 394, reduce the inductance of the coil? It will reduce the risk of flashing between adjacent layers by diminishing the maximum P.D. between adjacent parts of them, but it cannot have any effect on the rise of P.D. between the ends of the coil, or on the spark at the switch, on breaking the circuit. Is the author not thinking of the method of winding the coil with smaller wire in several sections which are afterwards joined in parallel? This, of course, would be hardly practicable for the shunt coils of a dynamo, owing to the greater space occupied.

Mr. Mavor.

Mr. H. A. MAVOR (*communicated*): On page 338 Mr. Esson states that the active belt of the machine becomes more active as the speed is reduced, and that he finds little agreement in the output-coefficient between the machines of different makers. There appears to be a little confusion in terms. Mr. Esson's output-coefficient is the reciprocal of $\frac{\text{watts}}{D^2 L R}$. Now I have never stated that I found this coefficient the same between machines of different makers. To make comparison on the lines which I suggested, the depth of slot has to be taken into consideration. If Mr. Esson has the particulars of the machines referred to and will introduce this term into his calculation, I think he will find that the aspect of matters will be somewhat changed.

Professor Silvanus Thompson, in a paper read to the British Association in Belfast, stated that he had examined a large number of machines, and found the results in substantial agreement with the value given to the constant used in my paper, read to the Glasgow Engineering Congress, 1901. The higher value of the output-coefficient on slow-speed machines is probably due to the use of a deeper slot.

Probably the members have not yet had before them a paper read by me to the Glasgow Local Section at their meeting last month, in which it is shown that the energy generated in the active belt, while fairly constant in large machines, is not so in sizes below about 20 ins. in diameter, owing to the increased relative importance of the iron losses in the core. If Mr. Esson could tabulate for the records of the *Institution* the following information with regard to the machines to which he refers, it would be useful:—

Total watts output.	Revolutions.	Mr. Mavor.
Diameter of core.	Span of poles.	
Length „ „	Flux-density in the air-space.	
Slot depth.		

Of course Mr. Esson is aware that to make a sound comparison between machines it is necessary to specify efficiency and temperature rise. The value of the constant which I adopted in my Congress paper is of course affected by both efficiency and temperature rise.

Referring now to page 352 in Mr. Esson's paper, he states that there is a best weight of copper relatively to the other parts of the machine. My contention is that this best weight of copper may be determined most readily by an examination of the relation between copper and iron in the active belt, and, as Professor Thompson has pointed out, the constant which I used is convertible into ampere-turns per unit cross-sectional area of active belt, so that if this constant be determined by calculation or found by experience, this is exactly what Mr. Esson is looking for. The paragraph to which reference is here made is under the heading "Alternating-Current Generators," but it is none the less true of continuous-current generators, which are of course also essentially alternators.

Mr. V. A. FYNN (*communicated*): Referring to Mr. Scott's paper first, I will divide my remarks into sections:— Mr. Fynn.

(1) *General Contour:* Mr. Scott's predilection for casting yokes in one piece and afterwards slotting or sawing these through is a very expensive one, and surely unnecessary. There is no difficulty whatever nowadays in obtaining either cast-iron or cast-steel yokes of sufficiently uniform quality; it must be borne in mind that it is good practice to work with fairly low densities in that part of the magnetic circuit, and for that reason it would require very bad castings indeed to make an appreciable difference.

(2) *Poles:* Laminated poles are a distinct advantage and should certainly be used whenever possible; when pole-shoes are reduced in section in order to obtain higher saturation, good results can only be hoped for when that saturation is carried very high to, say, 19,000 or 20,000 lines per cm². It then becomes a question whether this arrangement does not require more ampere-turns than does the usual practice of saturating the teeth, as the path will be longer; the advantage obtained is in any case only a fraction of that due to the air-gap itself, and in order to take full advantage of the armature the teeth must be saturated; the total benefit derived is therefore very questionable. I have never found that a slot which does not cut pole and yoke clean in two is of any use to prevent cross-magnetisation. Machines which require adjustable pole-tips in order to commutate sparklessly rely on the leakage-field from the pole-tips, and cannot therefore commutate sparklessly at all loads with fixed brushes. A modern machine should not be designed on these lines, but should be figured to commutate sparklessly in the neutral zone, in which case all that is to be guarded against is a distortion of the field sufficiently extensive to disturb this neutral zone materially. This object is not difficult to obtain, and may

Fynn. easily be achieved by symmetrical arrangements allowing of equally good performance when running in either direction and either as motor or dynamo.

(3) *The Armature Core:* Mr. Scott's proposal gradually to decrease the diameter of the plates in order to avoid the straggling appearance of the ends involves great risk of breakdown; a number of very sharp corners are thus formed which can easily cut through to the wire, especially as the plates, not being compressed tightly at those points, are liable to move. The teeth should all be well supported to within, say, a ¼ in. of the armature circumference. Brass end-plates answer the purpose very well. It seems to me that plates should either be slotted, annealed after slotting and assembled, or assembled in the shape of discs and the slots then milled out. A combination of both methods is surely waste of time, as it involves setting for both operations.

(4) *Shape of Slot:* I must differ from Mr. Scott when he says that the shape of the slot is not so much a question of electric dimensions as of machining. The dimensions of a slot have the greatest influence on the proper commutation of a dynamo, and all we can do is to satisfy mechanical requirements as far as commutating will permit. I am, of course, referring now to machines giving the greatest possible output for the least weight. In such machines closed slots or tunnels are out of the question. I can recommend from experience the method Mr. Scott now suggests for holding the wires down in the slots by means of metal wedges (his Fig. 13 E). I have used this very plan for the first time on a 150 k.w. machine designed in September, 1901, with the best results; the wedges were of brass.

(5) *Winding of Armature:* Even heavy section bars can be and are often partially formed; being bent at the pulley-end round a mandrel, anything conducing to a reduction of solder joints is to be welcomed. I feel certain that many people must have found that it was not necessary to bend both ends of each conductor in order to complete a winding, and most of them probably abandoned the idea because it doubles the amount of room occupied by the end windings, and adds some 50 per cent. to the weight of idle copper. Nevertheless, I know of instances where this style of winding has been used. Space and money were no object.

(6) *Internal Circulating Currents:* There is no doubt that parallel-wound or multiple-wound armatures are liable to get out of balance electrically; it is, however, no less certain that balancing connections or separate balancing windings offer a perfect cure when coupled with judicious dimensioning. As regards the series-parallel system of winding armatures, it has always been put forward by its votaries that its main advantage over the multiple system lay in its total indifference to inequalities of air-gap or pole-strength or other unbalancing influences. I see now, however, that notwithstanding all this, Mr. Arnold has taken out patents covering the application of balancing connections for these series-parallel windings.

(7) *Commutators:* The use of high resistance metal for commutator lugs has been greatly abused. That device is often of real value in order to make a machine less sensitive, but often also it is useless. It

is, however, easy to predetermine by calculation when benefit can be derived from it. Mr. Fynn.

(8) *Brush-Gear:* I am sorry to find that the author has said so little about brush-gear, that most vital part of a dynamo. I regret this the more as his ideas on the subject seem to differ from mine entirely. The brush-gear which I found to answer best is one in which the carbon block only moves, and that block should be positively connected by a flexible strip of ample section to the holder-body in such a way as not to hinder the free movement of the carbon. The blocks should be enclosed in a box. This makes the moving part as light as possible. There is no reason whatever for making the width of the carbon equal to a whole number of segments.

The author's ideas as to the design of direct-current machines must be very different indeed from mine, as I find that only the fewest of the figures which he gives as general come anywhere near my standards. Mr. Scott lays great stress on the relationship between ampere-turns required for air-gap and cross ampere-turns in relation to the commutating properties of a dynamo. In my estimation this relation has only a very secondary effect. I have designed several machines which will run absolutely sparklessly with full-load current at full speed when the voltage is reduced to $\frac{1}{10}$th of its normal value, in some cases even to less, and I find it quite easy to work with above relation at less than unity. I happen to have before me the data of a 65 k.w. dynamo of mine forming one of a series, which runs at 750 revolutions and commutates sparklessly with fixed brushes for all loads, the final temperature rise being 79° F. Although only a small dynamo, its constant for the "output equation" is ·0386, and compares therefore favourably with Mr. Scott's constant for flywheel generators. The "circumferential current-density" is of course high, but this figure just falls within the given limits, being 660, that is near the values given in the paper for very large generators. I think it is very necessary when setting up any such constants to differentiate between dynamos built for different voltages, and only compare machines showing the same temperature rise and commutating properties, to be really of value for the purpose of comparison, these constants should apply to machines similar in those respects. In addition, in order to be quite exact, the comparisons should be based not so much on revolutions per minute as on peripheral speed, as the output for a given temperature is closely associated with this figure.

(9) *Alternators:* The "tie rod" construction has certainly seen its best days, and is being now abandoned; in its place we have the far better "angle iron construction," which no doubt has come to stay, as it really solves a difficult problem, and I am surprised that Mr. Scott has omitted to mention it. With regard to the new Niagara Alternators and the foot-note relating thereto, it is also interesting to note that Mr. C. E. L. Brown had originally proposed an inner pole machine for that station, and has always been of opinion that it was the best type for the purpose.

(10) *Voltage Drop:* Mr. Scott omits to mention the armature-self-induction which is the principal cause for reduction of voltage at full

Mr. Fynn. load; the same considerations apply here as in the direct-current case. Surely the lower the periodicity required the easier it is to adopt slow speeds, and the higher the periodicity required the greater advantage is there in high speeds. To make the comparative figures in the tables of greater value the "regulation" properties of the alternator would have been of interest. This additional information would, no doubt, account for the apparent discrepancies. It is also not fair to compare alternators designed for a widely different periodicity and widely different terminal voltage. It has given me much pleasure to read Mr. Scott's very interesting paper, and it should call forth an instructive discussion.

Passing now to Mr. Esson's paper:—When speaking of the Frankfort Exhibition of 1891, and whilst complaining of the scarcity of alternators shown, Mr. Esson forgets to mention what was surely one of the features of the show, namely, the Brown three-phase machine. Although it is a design which, like others, has since disappeared, it did create at the time a great deal of interest and found numerous imitators. Had Mr. Esson noticed this alternator at the time, he would have found that there was practically no variation of flux for varying positions of the field, and he might have designed the improved alternators he brought out in 1895 several years earlier.

The statement that there is no essential difference between the self-induction of a coreless and an iron armature, I put down to a *lapsus linguæ*. Surely this point is clear enough at the present time.

I agree with Mr. Esson when he says that the "slider form" of brush-gear is most in favour. It is very justly so. I like to see the arm, however, as short as possible—in fact the whole appearance approximating that of the "reaction holder." The "hinged holder" type is decidedly faulty, and can only be used with some measure of success on commutators of very large diameter.

I notice that neither author has a good word to say for the so-called inductor alternator. Unfortunately time does not permit me to go into this matter very fully just now, but I may say that my experience with that type has been very satisfactory, and I have designed quite a number which in total weight are only very little behind the best heteropolar designs with all poles wound, whilst in cost they are distinctly more advantageous, comparing machines of equal drop. Commercial machines ought to have from 4 to 6 per cent. drop for a power-factor of one. I know of no alternators working in this country with only $2\frac{1}{2}$ per cent. drop at full load, and until quite recently our alternators here showed all a drop of nearer 12 than 5 per cent. The so-called inductor alternators are, however, not suitable for outputs exceeding, say, 2,000 k.w. at 1,000 revolutions as three-phasers, the main exciting coil becoming unwieldy in larger sizes.

Let me hope, in conclusion, that the downfall of the single-phase system, surely the simplest in existence, is not so near at hand as Mr. Esson's concluding remarks may lead one to believe. The new single-phase motor which I have designed, and which is being now thoroughly tried, may, I hope, be of great help to those stations who at this moment are only waiting for sufficient capital to change over to two- or three-phase, sometimes even to direct current.

Mr. H. LEDWARD (*communicated*): With regard to the output-coefficient of Mr. Esson, I should like to make a few remarks. From the formula for the voltage of a continuous-current machine, it is easy to develop the following formula (using inch units)— Mr. Ledward.

$$\frac{\text{K.W.}}{D^2 L R} = 1{,}67\, a \,.\, B \,.\, AS \,.\, 10^{-10},$$

wherein a is the ratio of pole-arc to pole-pitch, B is the flux-density on the face of the pole-shoe, and A S is the number of ampere conductors per inch of periphery. This formula, it should be noted, is not empirical, but is fulfilled absolutely in all machines. The output-coefficient is hence dependent upon the values of a, B, and A S, which are in turn fixed by the necessity for sparkless commutation and are independent of the frequency. The explanation given by Mr. Esson for the higher output-coefficient of Continental machines is therefore incorrect. The fact is that they customarily go a little higher with the value of A S and, perhaps, B. I do not, moreover, think that Mr. Esson is correct when he states that the frequency of Continental machines is lower than that of English machines; if anything, the reverse is the case. The average value of c for twenty machines (of Continental make) varying in output from 55–1,000 k.w., I have found to be 16,85. The maximum and minimum values being 35 in the case of a 100 k.w. machine of V.E.A.G.-Wien, and 7 in the case of a 350 k.w. machine of the Soc. Alsacienne.

With reference to the output-coefficient, I may mention that the output-coefficient of an alternator is—

$$\frac{\text{K.W.}}{D^2 L R} = 1{,}67\, a \,.\, B \,.\, AS \,.\, k \,.\, 10^{-10},$$

where k is the E.M.F. wave-factor = 1,11 for sine wave-form. I do not think that Mr. Esson is correct when he states that the output-coefficient of Continental alternators is about 0,015. I know machines the output-coefficients of which vary from 0,036 to 0,008, the first being a 300 k.w. of the Soc. Alsacienne, and the latter a 275 k.w. machine of Brown, Boveri & Co. This variation is, I think, considerably greater than is the case with modern continuous-current machines. For example, from 10 c.c. machines varying from 330–1,000 k.w. output, the output-coefficient varies irregularly from 0,053 to 0,026.

With regard to transformers, the shell type is undoubtedly cheaper and more efficient, and with a reasonable system of forced draught cooling, such as is employed by the Westinghouse and the General Electric Company, they are preferable to the core type for all large sizes (say above 100 k.w. per phase). They cannot, however, be easily built for more than one-phase, which explains the fact that the Americans prefer three single-phase transformers for three-phase work.

I think that one reason for the use of the special type of rotor winding mentioned in Mr. Esson's paper is that the ordinary type of squirrel-cage winding is patented by the A.E.G. in Germany.

Mr. E. V. CLARK (*communicated*): In connection with speed and periodicity, it is perhaps worth while to call attention to the fact that, Mr. Clark.

Mr. Clark. for a given periodicity, the linear distance between pole centres is dependent on the peripheral velocity of the machine, and on nothing else. This proposition is self-evident directly it is looked into, though I cannot recall having seen it in print. At 50 cycles, for instance, each armature bar must pass over 100 poles per second. If, then, the poles are 1 foot centres, the peripheral velocity must be 100 feet per second, or 6,000 feet per minute; and one can only reduce this to 5,000 feet per minute by crowding the poles in to 10-in. centres. As high speeds are associated with high peripheral velocities, it is in this class of machine that one finds the poles most widely separated.

In this connection, it may be pointed out that Mr. Esson is very conservative in fixing 6,000 feet per minute as the maximum advisable limit (*vide* p. 340). The E.C.C. 1,500 k.w., 50 cycle two-phase alternators in the new Leeds generating station run at 200 revolutions per minute; and the magnet-wheels, 13 feet 6 inches in diameter, have the great velocity of 8,480 feet per minute, with a polar pitch of almost 17 inches. It is almost impossible (except with low periodicities) to avoid a high peripheral velocity in machines of large output, simply on account of the crowding of the poles which otherwise results, and the inefficient use of materials when the magnetic path is several times as long as it is wide; and of this, the 3,500 k.w. Kolben alternator at the Willesden station of the Metropolitan Electric Supply Company, Limited, affords a good illustration. This machine, of which particulars have courteously been supplied me by the company's secretary, runs at 75 revolutions, generating two-phase current at 60 cycles, and its armature is 300 inches in bore, and 29½ inches in axial (core) length, the output-coefficient being ·0176—considerably smaller than one might have expected at first sight. But Mr. Eborall mentioned that the design of this machine was rendered far from easy by the high periodicity, and a little scrutiny will fully bear this out. Even with this large diameter, giving a peripheral velocity of about 5,900 feet per minute, the polar pitch is under 10 inches, and this with an axial length of pole of 29½ inches or thereabouts. In antithesis to this, we have the three-phase machine of the same speed and output, but generating current at 25 cycles, built by the General Electric Company of America, and illustrated in Fig. 28 of Mr. Scott's paper. Here the diameter over poles is reduced to 200 inches—but two-thirds that of the Willesden alternator—with a reduction in peripheral velocity to 3,900 feet per second, but nevertheless the polar pitch, on account of the low periodicity, is 15·6 inches, affording a much more satisfactory and efficient shape to the section of the magnetic path. The design of a 3,500 k.w. alternator at 75 revolutions for a periodicity of 100 is difficult to conceive. In fact, it is doubtful if any maker would be anxious to undertake to construct a machine of this output and periodicity, except perhaps for coupling to a steam turbine.

Mr. Bowen. Mr. H. V. BOWEN (*communicated*): I have read Mr. Scott's paper with great interest. Referring to the section "Winding of Armature," it would have been of great interest to many English designers if Mr. Scott had extended this section and favoured us with his experience and criticism of some of the uncommon Arnold Series Parallel Windings,

which no doubt he would have met, being so closely associated with the International Engineering Company. It is well known that these types of windings are very much in vogue on the Continent, particularly employed by German manufacturers. It would be interesting to know if these types of windings, arranged on slotted armature cores and without balancing rings, are altogether a success as regards sparkless commutation and the absence of internal circulating currents, assuming a normal ratio of armature ampere-turns to field-ampere turns. To clearly indicate the type of series parallel windings I refer to, I give the following data of some dynamos of various German firms, the first two being taken from Table I. Mr. Bowen.

EXAMPLES OF ARNOLD'S SERIES PARALLEL WINDINGS.

Voltage.	K.Ws.	Revolutions per minute.	No. of Poles.	No. of Armature Circuits.	No. of Lines of Brushes.	Total No. of Conductors on Armature Periphery.	No. of Segments in Commutator.	Pitch of Armature Conductors.	No of Slots on Surface of Core.
550	1,000	95	14	10	14	1,144	572	81	286
550	350	94	12	6	12	1,218	609	101	609
550	165	400	8	4	8	500	250	63	250
500	650	90	30	10	30	2,020	1,010	67	505

The last machine is very interesting, as it possesses several uncommon features. It is a direct-coupled steam set, and literally a flywheel type generator, the armature core being built on the top surface of the heavy cast-iron flywheel. The machine was manufactured by the Helios Company, Köln, Ehrenfeld, for operating machinery in a cement works in Hanover, I believe. The following are some of the dimensions :—

Diameter over the armature stampings ...	5,000 mm.
Width over core stampings	210 ,,
Diameter over the field system, exclusive of the extra for yoke feet	6,100 ,,
Diameter of Commutator	3,000 ,,

There are 505 slots on the surface of the core, each 16 × 42 mm., and there are four armature conductors per slot, each 4·5 × 12 mm. The brush gear is supported from the yoke by four brackets.

I should like to ask Mr. Scott if he has met with such an uncommon example with Arnold's Series Parallel Winding, and if so, his critical remarks upon the observed behaviour of the machine.

Referring to these types of windings, I have in mind two 100 k.w., *230 volts, 6 pole generators,* both having series parallel type of armature

Bowen. winding. These machines were built by a prominent German firm, and who are at the present time offering very cheap machines upon the English market. These two machines, even when running at half load, became very hot on the armature, and sparked badly. Probably the risks attached to these types of windings have prevented English and American designers from adopting them, and it would be very interesting, I am certain, to the members of the Institution who are in touch with the designing of generators, to hear Mr. Scott express his opinion upon these uncommon windings.

On page 379, Mr. Scott states that "in ordinary parallel winding the number of slots and commutator segments must be even, whereas in series winding the numbers must be odd." If the armature be loop wound, a parallel winding will result, no matter whether the number of commutator bars be even or odd; and again, in two circuit series windings, if the number of pole pairs for the magnetic circuit be odd, a series winding can be obtained by an even number of commutator segments, as shown by the data given below of a 6 pole 75 k.w. generator, which I designed some time ago.

On page 400, and in the last paragraph, Mr. Scott states that if the "circumferential current density" is about 700, it indicates a skimped design. I would like to point out that this figure must be reached, and sometimes must be exceeded if the machine is of large capacity, running at a high speed, otherwise the iron losses will far exceed the C^2R losses of the armature, with the result that the all-round efficiency will be reduced; so that under certain conditions this figure cannot indicate a skimped design. Of course, in the case of generators which run as shunt machines at 460 volts for lighting purposes and 550 volts for traction purposes, with a constant kilowatt output and speed, this circumferential density must be reduced on account of the weak field and large current under the shunt conditions and the consequent introduced sensitiveness of the non-sparking range.

On page 403, Mr. Scott states that the average figure for the amount of current which can be collected at one pole can be taken at 500 amperes per pole. This figure, I think, would be high for high-speed traction generators, and I am afraid that in the majority of cases, if traction and lighting generators with slotted cores were designed on these lines, their behaviour would not be altogether desirable as regards sparkless running with fixed brush position on variable loads.

On page 404 are given some extracts from Messrs. Parshall & Hobart's book on Electric Generators, in which the gap ampere turns divided by cross ampere turns are between ·61 and ·68. With such a ratio as this, and if the machines were shunt-wound and self-exciting (the generators as a matter of fact being compound-wound), the variation in terminal voltage through varying loads would be considerable.

I may state that one of the manufacturers of these machines, according to one of their recent designs, has now adopted a ratio of over unity. This ratio would be a far better factor for indicating skimped design than "circumferential density." A ratio less than unity really means a machine very much overloaded.

Table I. supplies much interesting information with respect to

various makers, representing different characteristics and constants of design, and I give below data of a few machines which I have designed for the Industrial Engineering Company, of Newton, Hyde, at various times. These figures may be of interest to those perusing Table I. Mr. Bowen.

DATA OF THREE HIGH-SPEED DIRECT-COUPLED "INDUSTRIAL" GENERATORS.

	K.W.	550	250	75
	Amperes	1,000	770	137
	Volts	550	325	550
	Revs. per minute	330	380	500
Winding	Armature	Drum	Drum	Drum
	Field	Compd.	Compd.	Shunt
	No. of Poles	8	8	6
Armature	Diameter of Armature	54	44	28
	Length of Core	11½	10½	6½
	No. of Slots	168	192	131
	Core Section	180	120	68
	No. of Conductors	1,008	768	524
	Section of Conductors	·075	·065	·03
Field Magnets	Section of Pole	140	90	53
	Polar Surface	170	130	72
	Air Gap	7/16	9/32	3/16
	Section of Yoke	190	125	72*
	Poles	Solid	Solid	Solid
Commutator	Commutator Diameter	36	30	19
	Commutator Face	12	12	4
	No. of Segments	504	384	262
	No. of Brushes	8 sets each of 8	8 sets each of 8	6 sets each of 4
Weights	Armature, Copper	1,060	630	150
	Field, Copper	2,300	1,650	540
	Commutators, Copper	1,000	860	200
	Armature Core, Iron	3,500	2,000	700
Net Efficiencies	Full Load	95·5	93·5	92·5
	¾ Load	95	93·0	92
	½ Load	93·5	91·25	90
	Circumferential Density	750	540	420

* All the yokes are of cast iron.

Note.—All dimensions and weights in inches and pounds.

The working characteristics of the machine are quite satisfactory in every respect, the temperature rise keeping well within the usual limits, and no sparking taking place from no load to 25 per cent. overload, with fixed brush position, and further capable of taking 50 per cent. momentary overloads without injury.

Mr. W. B. ESSON: I am glad to say that in replying to the various criticisms that have been passed upon my paper my task is a comparatively light one. The first speaker, Professor Carus-Wilson, re- Mr. Esson.

Mr. Esson. ferred to the question of commutation, and it would seem with regard to this matter that the more we know the less we really learn about it. We used to think some ten years ago that we knew about it all there was to be known, after the classical papers of the late Dr. John Hopkinson and others had been read. There had to be a fringe of magnetic lines at the pole-tips which allowed the current to be gradually stopped in each section as it came under the brush and induced in it a current in the opposite direction just of the proper magnitude before the section left the brush. The latest exponent of the American view is, I suppose, Mr. Hobart, whose excellent paper read at the Glasgow meeting is still fresh in our memory, but so far as I am aware Mr. Hobart does not find fault with the old theory. The argument, I take it, is this. You probably want some kind of fringe for bringing the current in the section to zero and reversing it, but the magnitude of this fringe must be dependent on the reactance voltage. Make the reactance voltage negligible and then you can be independent of the fringe, or in other words you can do without a positive field and work, as Professor Carus-Wilson says, even with a negative one.

In some of his dynamos I understand Mr. Hobart has a reactance so small that he can run them with a trailing instead of a leading brush, thus making use of what would otherwise be the back induction of the armature for strengthening his field. In this case it becomes, of course, forward induction. I have pointed out in another place[1] that when the armature teeth are highly saturated, the *real* air-gap by no means corresponds with the *apparent* air-gap, so it is just possible that in the cases cited by Professor Carus-Wilson the field under the pole-tips is not really but only apparently reversed. However that may be, it is certain, as the speaker says, that without carbon brushes we could not in modern machines get the results we do. As Mr. Hobart points out in the paper referred to, the idea of reactance is not new, and when the matter is looked carefully into, reducing the reactance voltage really means making the commutator sections for a given machine as numerous as possible. In large output machines where we are reduced to two bars per commutator section, the current in each bar must be restricted to about 150 amperes, and the number of the poles must be settled so that this current per path is not exceeded.

Coming now to Mr. Eborall's remarks, I understood him to say that the output-coefficient for a consistently designed line of machines varied in a perfectly regular way. I quite agree with Mr. Eborall that if there is variation, this should be and would be regular; at the same time there may be no variation at all in the coefficient, as witness the line of machines proposed by the late Professor Short to which reference is made in the paper. All these machines have a similar output-coefficient from the highest to the lowest. In a consistently designed set of machines, then, the line connecting the size of the machine with the output-coefficient may vary from a straight line to a curve of regular form according to the idea of the designer, but consistency is not of course to be expected when comparing machines by different makers taken at random.

[1] *Journal of the Institution of* Electrical Engineers, vol. 31, page 239.

Mr. Esson.

The above remark answers some of the observations made by Professor Silvanus Thompson, who complains about the variation in the output-coefficient. Talking of the Steinmetz coefficient, I have never used it or felt the need of it, due no doubt to my attacking the design from a starting point which did not make its application convenient. I do not agree with Professor Thompson when he says that my remark that "the so-called active belt becomes more active as the speed is reduced" is misleading, because this remark has reference to an armature of given size, and it is plain, therefore, that in this case speed is synonymous with surface velocity. Again, the d^2l coefficient expresses quite correctly the output according to the product of the total surface and the surface velocity, though for convenience the equation is expressed in terms of revolutions per minute. It is merely a matter of substituting a term for surface velocity instead of diameter × revolutions per minute and altering the coefficient to correspond, but as every one talks of revolutions per minute, in the form given the expression appears more serviceable. Whichever is preferred, the fact is not altered that as you reduce the speed of a given armature you can press up the induction and so increase the output for the same heating, which brings us back to where we started, namely, that with reduced speed there is more activity in the active belt. It can be shown with regard to this activity that the condition for getting maximum results is that the weight of the iron in the core teeth must be about equal to the weight of the copper in the slots.

Mr. Barker's remarks on turbine generators are extremely interesting, but I do not think that weight *per se* has much to do with the matter. Once erected in a generating station, the set has not to be shifted annually for the spring clean, so that whether it weighs 85 or 400 tons is only of importance in that it may affect the cost. The kind of workmanship in a turbine is different to that in an ordinary engine, and the real question as to superiority will be decided by the cost of the two classes of machinery in the long run. That the turbine has attained great success is undoubted, and one cannot regard with too much admiration the man who in the face of scorn, opposition, and ridicule has by his persistent effort and mechanical genius brought this machine to its present high state of perfection. All this I may say while confessing that turbines always get on my nerves. When I enter a station where turbine generators are running I have only one desire—that is to get out again. The noise and general air of feverish restlessness about the place worries me, whereas when I go into a station equipped with slow-speed plant, where fine engines are running with quiet and dignity, there is a feeling of calm about the whole which strongly appeals to me. That, however, may be merely a matter of temperament.

I don't approve of the tandem dynamo, if Mr. Barker means by that two or more armatures threaded on the same end of one shaft. I do approve, however, of a dynamo on each end of the shaft, *i.e.*, at each side of the engine, which is a very different thing. Dynamos of the first class are only to be tolerated under special conditions, and I would not be surprised to find them ruled out of court by competent engineers whenever these conditions are not present.

Mr. Esson

I think that I am right in saying that the active belt of the machine becomes more active as the speed is reduced, because the induction in the belt can then be put up and the output depends on this; but Mr. Mavor is quite correct in pointing out that he never stated that the coefficient I use had the same value in different machines. Perhaps we regard the use of coefficients from a different standpoint. In my view their chief uses are two: first to determine quickly the carcase dimensions of a new machine so that we can get out an estimate of its cost, secondly to see if we are getting from a machine of given carcase dimensions the best it can do. Now, any constant which embraces a term depending upon the depth of the slots is, to my mind, useless for these purposes, as this depth has nothing to do with the weight of the machine upon which its cost depends. Mr. Ledward's remarks I do not understand. He seems to have discovered a mare's nest, and fails to understand that the several values upon which the output of machines depend can be massed together in one term which, in conjunction with the carcase dimensions of the machine and its speed, serves all purposes.

Coming to alternating-current work, Professor Silvanus Thompson points out that in modern alternators the pole-pitch is practically uniform. That, of course, cannot be otherwise if the frequency in the several cases is the same and the peripheral speed is also similar. It will be seen from the figures for alternators in Mr. Scott's Table VII. that the peripheral speed generally lies between 5,000 and 6,000 feet per minute, the former at 50 frequency corresponding to a pole-pitch of 10 inches and the latter to 12 inches. Professor Thompson knows very well, of course, that the voltage, efficiency, drop, and specific utilisation of material have nothing to do with this point, it depends entirely on the peripheral velocity and frequency; at the same time, like Professor Thompson and Mr. Clark, I do not remember uniformity of pitch having been referred to in print before. But when Professor Thompson endeavours to base on this mere accident a general principle of design, it is time to call a halt, for a moment's reflection will show him that his proposition as to circumference of working face is untenable. Imagine a machine of 100 frequency with a surface velocity of 10,000 feet per minute! And yet we are called upon sometimes for machines of 100 frequency.

Mr. Clark remarks that I am very conservative in fixing 6,000 feet per minute as the maximum advisable limit of magnet velocity. I may be conservative, but I would not say "very," for in modern machines it is rarely that this velocity is exceeded. I have myself gone up to 7,920 feet on special occasions; the machines are running quite satisfactorily, and up to the present nothing has burst; at the same time I think, as mentioned in the paper, that 6,000 feet is quite high enough, and reference to Mr. Scott's Table VII. will show that in no case is this exceeded in the alternators he cites.

Mr. Eborall referred to the better qualities of high-speed alternators as compared with those of low speed, but there is one point he did not refer to, and that is, that on account of each element of the machine—consisting of a set of armature coils and pole—giving in the high-speed

generator a greater output it is more efficient. In the low-speed machine we have a great number of elements each giving a small output, in the high-speed machine a small number of elements each giving a comparatively large output—hence the greater efficiency of the high-speed machine. Mr. Esson.

I quite agree with Mr. Eborall as to forced draught for transformers in preference to oil, and I entirely disagree with Mr. Ledward as to the shell type being either cheaper or more efficient. But as regards the Heyland motor, Mr. Eborall knows as well as I that in non-synchronous machinery the commutator constitutes the one element of impurity which it is our sole aim and endeavour to avoid. From this point of view, therefore, the Heyland motor is not a purely inductive machine, hence my describing it so in the paper. With reference to the drop in alternators, I did not say that 2½ per cent. was attained in practice, but that trade representations were made to this effect. Mr. Eborall will know, of course, that there is a difference—sometimes. To the statement that English machines were far too heavy until recently, I must still adhere. No doubt 2½ per cent. drop would be better than 4 per cent., other things being equal; but other things would not be equal, inasmuch as the cost for the small drop would be prohibitive, and, as Mr. Eborall points out, the parallel running would be bad. Accordingly, after showing that the small drop is desirable, my critic goes on to show it is unnecessary; and as that is just the conclusion arrived at in the paper, the matter may be left there. One other point mentioned by Mr. Eborall has reference to two-phase and three-phase motors, and he states that the former are inferior in output, power-factor, and efficiency to the latter. I think if he looks into this matter he will find that the three-phase motor requires more copper than the two-phase, which would probably account for the difference.

With Mr. Sparks' observations on the subject of "Conversion" I agree generally, but it was not my intention on the occasion of the discussion on Mr. Eborall's paper to indicate difficulties in controlling the pressure when non-synchronous motors were employed. I merely pointed out that certain precautions had to be adopted in controlling the pressure, which is quite a different thing. When these precautions are taken no difficulty whatever is experienced, and for every kind of machinery the regulating gear suitable to its use must be installed if satisfactory results are to be attained. With these remarks I thank you all for receiving my paper so kindly.

Mr. E. KILBURN SCOTT, in reply: Referring to the conditions necessary for sparkless commutation, Prof. Carus-Wilson shows that the method I give of comparing ampere-turns of air-gap with cross ampere-turns, as well as the reactance voltage method of American designers, both fail to meet the case. Even with these incorrect methods, however, the dynamo has developed into a fairly satisfactory machine, helped, no doubt, by the use of carbon brushes, saturation of iron round the air-gap, and in traction generators by the *over-compounding.* At the same time it would be most interesting to have the results of a series of tests such as Prof. Carus-Wilson has hinted at. Mr. Scott.

Mr. Eborall objects to the item in my standard specification of

Mr. Scott. alternators, dealing with the number of slots. In mentioning two slots. I was thinking of average-sized machines. For very large outputs, more slots would of course become *economically* possible from the manufacturing point of view.

Regarding the comparison of two- and three-phase machines, I must confess to having considered the matter in a very general sense, namely, that the three-phase armature has all its slots occupied, and the two-phase only two-thirds of the slots. I know that in practice this relation is not quite followed, and that questions of cooling surface, etc., come in to limit the output.

Mr. J. H. Barker is wrong about the Metropolitan Railway turbo-alternators, for I saw them recently under construction at the Trafford Park Works.

Late delivery of dynamo cast steel certainly does handicap many English firms. On the Continent there is not the same difficulty, because several concerns make a speciality of such steel. Messrs. P. R. Jackson & Co. and the Westinghouse Company, who make their own steel, have a big pull, as they not only get immediate delivery, but the steel is to standard quality.

Single-core plates are thicker than segments, but as the latter have to be arranged in pairs, metal to metal to break joint, there is not much in it.

Mr. C. C. Hawkins rightly points out that the maximum output obtainable from any given carcase and quantity of copper and iron, is the information most desired. Unfortunately designers generally prefer to keep such data to themselves. Where firms design very closely to the specified requirements, it is a good deal due to their employing standard carcases and thoroughly testing every new size. In this country I am afraid the testing department is not in close enough touch with the designing and drawing offices, and I have also noticed a laxity in entering up the particulars of *such few tests* as are made.

Armature conductors with a single bend take up more room, but this becomes of less importance the greater the number of poles. I certainly think that anything which does away with bands and wedges to hold the conductors in position is a step in the right direction.

Regarding carbon brushes, I object to loose sliding carbon blocks for large dynamos because of their loud chattering noise.

Vacuum drying is principally employed for drying out the coils, but after they are assembled the completed armature may also be treated. I do not think there is any more difficulty in removing the last traces of moisture than with ordinary baking.

Prof. S. P. Thompson's elaboration of the output equation is instructive, and his further remarks on equalising rings will help to draw attention to the necessity of making provision against inequality of air-gap and magnetic value of the poles. I note that the Westinghouse Company guarantee that by their method the armature may be as much as $\frac{1}{16}$ inch out of centre without detriment.

I had not heard of water-glass as an insulator of core discs; its refractory character should give it additional value.

That the pole-pitch of alternators averages $9\frac{3}{4}$ inches for a

frequency of 50 ∾ is a useful fact to know. Curiously enough the same peculiarity is referred to in Mr. E. V. Clark's remarks. Mr. Scott.

Mr. V. A. Fynn thinks it unnecessary to cast the yokes in a solid ring, and if one could be sure that the two halves were run from the same ladle of metal it would not matter. When two halves are cast together the quality *must* be the same. If the armature end plates have fingers cast on to support the teeth, then the core discs need not be reduced in diameter at the corners. Unless the teeth are fairly large, however, such fingers may snap off.

The fact that the figures I put forward differ so much from those found useful by Mr. Fynn, shows the necessity of ventilating this question of size for a given output. I believe greater outputs could be obtained per pound of iron and copper than is at present the case with many English machines.

Prof. David Robertson's remarks form quite a treatise on armature windings, and I am pleased to have his corrections and criticisms. The windings *actually* employed in the workshop are few, but it is necessary to have some law or equation governing all types such as Prof. Robertson has given. His suggestion of machines having two commutators is interesting, but too expensive. As pointed out in Fig. 17, there has been a slip in making the block.

Mr. H. V. Bowen refers to the Arnold series parallel winding. There is no doubt that for all ordinary-sized machines this winding has been very successful. I have not, however, had an opportunity of observing its effect on such an extreme case as a 30-pole dynamo at 90 revolutions per minute.

Possibly the two 6-pole generators mentioned were overrated in output. Much overrating goes on, and is not confined to Continental makers. Some rules such as the American Institution of Electrical Engineers have adopted would go far to reduce the practice.

I am glad Mr. Bowen has given the additional data of his three multipolar dynamos; it is a much needed example to other designers in this country.

In conclusion I have to thank all those who have contributed to the discussion for their critical and interesting remarks. Regarding features which I may seem to have put forward as novel, I might just say that the paper was written two and a half years ago. In giving the tabulated data and dimensions, etc., I wished to put on record certain particulars which I thought might be useful to others. I think they may be taken as fairly representative.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Members.

Wilson S. Carr. | William Herbert Donner.

Robert Hood Haggie.

Associate Members.

Hugh Bourne.
George Alfred Brade.
Lawford Stanley Foster Grant.
Walter Charles Heavysege.
Henry Norman Holland.
Arthur C. Johnson.
Sidney Mellor.
William Duke Palmer.
Charles Edmund Pecsenik.
Robert Edward Robson.
Sydney Aston Mersey Rose.
George Ernest Sanders.
John Edward Tapper.
William Izett Walker.
Charles Stevens Ward.
John Henry Watkins.
Ettrick Lovell Webb.

Associates.

Rupert Stanley Allen.
Fountaine M. Burrell.
John Maldin Harvard.
John Henry Hopton.
William Day Kirkpatrick.
John Spencer.
Norman Wells.

Students.

Percy Everett Banting.
Stephen Donald Barnwell.
Edgcumbe R. Brighton.
F. H. Brun.
Albert Dixon Forster.
WilliamJames Freeman-Horn.
Bertram Barrett Grace.
George Cuthbert Hatton.
Herbert Hayhurst.
Franklin Thomas Homan.
Edward W. Jackson.
Richard Pelham Jephson.
Thomas Frederick Lee.
Stanley L. Burnett Lines.
Charles J. Cuthbert Moon.
R. C. Plowman.
Herbert Sidney Plymen.
Harry Stansfeld Porter.
Rowland Rees.
John Duncan Reid.
Clive Smith.
Frank Fawcett Smith
Wilfred Wallis Soutter.
Roland Francis Thomas.
John Turnbull Tiplady.
Laurence R. Wallace.

No. of Certificate 18,393.

N. L. 17,801.

THE COMPANIES' ACTS 1862 TO 1900.

[COPY.]

SPECIAL RESOLUTION

(Pursuant to the Companies' Act, 1862, Sections 50 and 51)

OF

The Institution of Electrical Engineers.

Passed February 26, 1903, *Confirmed March* 17, 1903.

At a SPECIAL GENERAL MEETING OF THE MEMBERS ONLY of the above-named Institution, duly convened, and held at the Institution of Civil Engineers, Great George Street, in the City of Westminster, on the twenty-sixth day of February, 1903, the following SPECIAL RESOLUTION was duly passed ; and at a subsequent SPECIAL GENERAL MEETING of the Members only of the said Institution, also duly convened and held at the Westminster Palace Hotel, Victoria Street, in the City of Westminster, on the seventeenth day of March, 1903, the following SPECIAL RESOLUTION was duly confirmed :—

RESOLUTION.

"That the Regulations contained in the Articles of Association of the Institution be altered in the following manner, that is to say :—

1. ARTICLE 38. By adding the following words at the end of the existing Article :—

> "In case any individual shall be adjudicated bankrupt, or convicted of felony, the Council may, without any such proposal or steps as aforesaid, decide that his name shall be removed from the Register of the Institution, and the Secretary shall communicate such decision to the individual according to the Form EE in the schedule."

2. ARTICLE 41. By adding the following words at the end of the existing Article :—

> "And the President shall be chosen from the Vice-Presidents and those who have been Vice-Presidents of the Institution, or predecessors of the Institution, and the Vice-Presidents shall be chosen from those who are, or have been, Members of the Council of the Institution, or predecessors of the Institution."

3. ARTICLE 42. By striking out the expression "one Vice-President" wherever it occurs in the Article, and substituting in each case the expression "two Vice-Presidents."

4. ARTICLE 50. By striking out the words "Immediate Past President and the four senior," and substituting the words "five junior."

5. ARTICLE 54. By inserting the words "and Associate Members" after the words "Special General Meetings of Members."

6. ARTICLE 56. By inserting the words "and Associate Members" after the words "*1st Special General* Meetings of Members."

7. ARTICLE 62. By adding the words "and Associate Members" after the words "vested only in a Special General Meeting of Members."

8. SCHEDULE FORM B. By adding the expression "aged " after the expression "A. B. "

9. SCHEDULE. By inserting after the Form E the following Form :—

"EE.

"The Institution of Electrical Engineers.

"Sir,

"It is my duty to inform you that on the day of the Council decided, according to Article 38, that you should be declared to be no longer belonging to the Institution.

"I am, Sir, etc."

10. SCHEDULE. By cancelling the Form G, and substituting therefor the following :—

"G.

"The Institution of Electrical Engineers.

"I, the undersigned, having been elected a of the Institution of Electrical Engineers, do hereby request to be registered as such and agree that I will be governed by the Rules, Regulations, and Articles of the said Institution as they now are, or as they may hereafter be altered; and that I will advance the objects of the Institution as far as shall be in my power; provided that, whenever I shall signify in writing to the Secretary that I am desirous of withdrawing from the Institution I shall (after the payment of any arrears which may be due by me at that period) be free from this obligation.

"Witness my hand, this day of ."

WALTER G. McMILLAN,
Secretary.

GLASGOW LOCAL SECTION.

THE DESIGN OF CONTINUOUS-CURRENT DYNAMOS.

By HENRY A. MAVOR, Chairman of the Section.

(Paper read at Meeting of Section, Nov. 11, 1902.)

Following up the paper on this subject read by the present author at the International Engineering Congress, Glasgow, 1901, there are some points to which he proposes to give further elucidation. As in the case of the former paper, it is not proposed to enunciate any new theories, or to discuss the bases of dynamo design. The present investigation has to do rather with the application to practical design of the results of the calculations and experiments of others. The generally accepted constants and formulæ may be by this means subjected to such criticism as to lead to modification of generally accepted views, and therefore such an investigation is not without its uses.

Referring back to the Congress paper, the author there suggested that the essential part of the dynamo is the region occupied by the armature conductors in the magnetic field, and suggested the name "Active Belt" for this region, which is bounded by the peripheral surface of the armature, the surface of the core at the bottom of the slots, and the ends of the core. He pointed out that an examination of the dynamo in terms of the energy generated in this active belt shows that machines of widely varying size, output, and speed, give a remarkably constant value in watts generated per cubic centimetre of active belt at unit velocity in unit field.

In an interesting paper read before the British Association in Belfast this year by Professor Silvanus Thompson, the professor gives the results of calculations on a large number of machines considered from this point of view, and confirms the value given in the Congress paper referred to. This value is often exceeded, but the statement originally made that 5 ergs per second per cubic centimetre at unit velocity in unit field may be counted upon as a safe load, is fully confirmed. The application of this method of study to machines of smaller sizes has shown that this value must be subjected to considerable modification, the reason being that in the smaller machines the losses in the armature are relatively more important than in the larger sizes.

On machines of 24 inches diameter and upwards it appears that the output may safely be given in watts per revolution, that is to say, that a given carcase will give an output directly, or nearly directly, proportional to the speed of rotation of the armature. In modern designs of large machines it is not difficult to determine this output, both with regard to the commutating conditions and to the temperature of the armature.

The design of the smaller machines, for which the market is very rapidly increasing, is not less worthy of close attention, and it would

appear, judging from the machines now on the market, that this part of the subject has not yet received the attention which it deserves. A very interesting series of articles at present appearing in the journal *Traction and Transmission*, by Mr. Henry M. Hobart, is dealing somewhat fully with this subject, and should be read by all interested in dynamo design.

In this connection it is interesting to note as a forecast of the probable development in this country, that in America the year's census of work, according to the American *Electrical World*, is 9,182 continuous-current machines of 428,601 H.P., giving an average of only 35 k.w. per machine for the year 1900.

Confining, as before, our attention to the armature, the points requiring consideration in the design of the machine arise primarily from the commutating conditions, and from the limit of temperature rise imposed. The commutating conditions on small machines can be fairly safely dealt with on the lines indicated in the Congress paper, and it will be found that this part of the question is much more easily solved than the other. The temperature rise in the armature is taken to be due to the following expenditures of energy on the material of the armature :—

(1) Hysteresis in the core.
(2) Eddy currents in the core.
(3) Hysteresis in the teeth.
(4) Eddy currents in the teeth.
(5) Eddy currents in the winding.
(6) C^2R losses in the winding.

These are the losses which may be expressed in watts, to be deducted from the effective watts of the machine.

The subject of hysteresis and eddy currents has been discussed with great fulness in various treatises on magnetism and on dynamo design, and it is not proposed here to enter into any discussion on these subjects, but to assume relations between those losses and the induction, periodicity, and weight. It is quite certain that the constants deduced from the study of these phenomena in the past must be subjected to some considerable modification, and the story is not yet told of all that takes place in a piece of iron under changing magnetisation and in a copper conductor in a changing magnetic field, but the constants now in general use have proved sufficiently near the truth to warrant their being used for our present purpose, and we may therefore follow to a logical conclusion the assumptions involved in those constants, reserving to ourselves the right to retrace our steps and amend our constants when they prove inconsistent with the results of experience. We are just as much warranted in doing this as we are in accepting the tests of tensile strength of steel and cast iron, and to all appearance the electrical data are even more reliable than the physical data to which we have referred.

It is customary to treat hysteresis and eddy currents in the core, and hysteresis in the teeth as being proportional to the periodicity, or the rate of change of magnetisation in that core, that is to say, to the speed. he eddy currents in the winding are usually assumed to be negligible

in machines in which the winding is embedded in slots and tunnels, and to be also of comparatively small importance on smooth-cored machines where the conductors are well laminated.

These losses appear as heat, producing a rise of temperature in the armature. The cooling effect of the circulating air round the moving armature tends to dissipate the heat, and a point is found when the heat production in the armature is balanced by the radiation from it, and the temperature becomes approximately constant. It is customary to specify this constant temperature at something between 70 deg. and 90 deg. Fahr. above the temperature of the surrounding air.

On small machines this is an exceedingly uncertain standard, because it is difficult to ascertain what is the temperature of the armature. It is difficult even to ascertain what is the temperature of the surrounding air. There is a further difficulty that a very insignificant variation in the condition or distribution of the material in a small machine very materially modifies the temperature rise. As, however, no more satisfactory standard of comparison has been generally adopted, we shall also assume for the purpose of the argument that it is practicable to forecast the temperature rise of the machine, and to measure it when it occurs. For practical purposes, speaking generally there is no very great difficulty in doing so. The exceptions which frequently arise are usually due to ascertainable abnormalities.

Assuming for the present that the proper constants have been ascertained for determining each of the losses, we may symbolise them by—

$A_{I}\, n =$ Hysteresis and eddies in the core in watts.
$A_{II}\, n =$ Hysteresis in the teeth in watts.
$A_{III}\, n^2 =$ Eddies in the teeth in watts.
$A_{IIII} =$ Watts lost in the winding.

The radiation of heat from the armature is taken as proportional to the square root of the speed.

The constants are determined for any given rise of temperature by experiment. The expression for the total lost watts radiated by the armature takes the form—

$$A_{V} \sqrt{n},$$

and the copper loss is ascertained by deducting the iron losses from the value $A_{V} \sqrt{n}$.

$$\text{Thus} \quad A_{V} \sqrt{n} - A_{I}\, n - A_{II}\, n - A_{III}\, n^2 = A_{IIII}$$

That the text of the paper be not unduly defaced by formulæ, they are relegated to an Appendix which gives constants and formulæ—(1) For the determination of core and tooth losses. (2) Radiation from the surface of the core. (3) The watts generated by the machine, its output, and efficiency.

The total watts generated in the winding of a dynamo, or absorbed in the winding of a motor armature, are directly proportional to the square root of the watts lost in the winding, the square root of total

cross-sectional area of the copper, the volume of the core, the induction in the air space, and to the rate of revolution and inversely proportional to the square root of the length of each turn of the winding.

$$\text{Thus} \qquad W = \frac{\sqrt{A_{iiii}} \times \sqrt{Q\,S_v} \times \pi DL\,B_a v \times 10^{-5} \times n}{\sqrt{2L + \frac{D\,7{\cdot}44}{P}}}$$

See Appendix III.

This formula for total watts symbolises the fact that the total output of the armature varies as the square root of the loss in the winding and directly as the speed. Now, if the loss in the winding be relatively small, the output will, for practical purposes, vary directly with the speed. This is the case in large machines. In small armatures, on the other hand, the winding loss is relatively large, and it is worth while to look for the conditions which give maximum output.

This can with a little labour be done for any machine by a simple graphic method.

Plotting the values of $\sqrt{n}$, n and n^2, multiplied by the relative constants, to scale we get a diagram for any armature giving the value of A_{iiii} (Fig. 1).

Again plotting values of W and n we get a diagram of the form shown in Fig. 2.

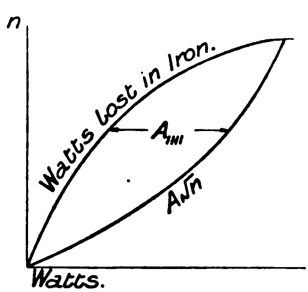

FIG. 1.

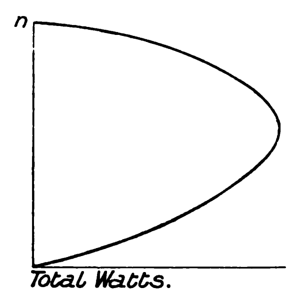

FIG. 2.

The form of this curve is interesting. It shows that there is for every machine a maximum possible output for a given rise of temperature, and that this maximum is at a definite speed of rotation. The position of this curve and of its maximum may be varied by several means.

For low speed the total watts and the efficiency are greater with a deep slot and high inductions.

For high speed the total watts and the efficiency are greater with a shallow slot and low inductions.

The ratio between the iron and the copper can be adjusted to suit the conditions of work to which the motor is likely to be subjected. If, for example, it has to run constantly with a varying load on outside

supply paid for by meter, it is evident that the iron losses should be kept low and the copper losses high; whereas if the machine is to run at full load, it should be designed to give the output under the most favourable conditions of combined iron and copper losses. Attention to this point results in a very considerable saving in cost of working.

A reference to the formulæ in the Appendix will show how the values of these losses have been determined. The losses in the core are seen to depend upon its weight, the core induction, and the periodicity of magnetic reversal. See Appendix I.

It is convenient to find expressions deriving the core induction from the induction in the air space, and this is contained in a formula given in Appendix I.

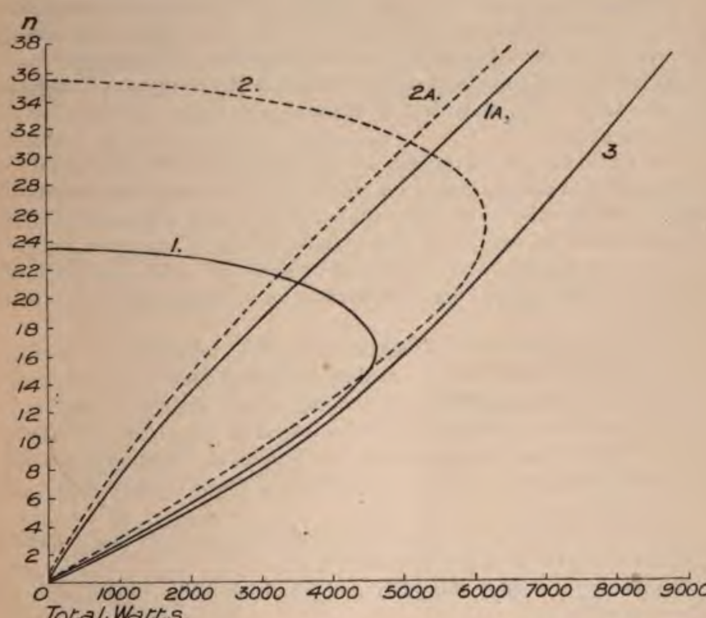

FIG. 3.

A high value of the induction keeps down the quantity of material in the active belt, the number of turns of wire required, and the reactance voltage. On the other hand, the induction in the air space is limited by the necessity for mechanical clearance and by the size of the carcase. The author has not gone into the investigation of the highest permissible value of the induction in the air space. This maximum will be found to depend with any given form of magnet on the relation between the length of the path through the magnet and the space available for wire.

It is now almost universal to demand a fixed brush position for all loads, *and therefore it is of* the highest importance that the values of

the induction in the air space and teeth should be kept as high as practicable.

The range of speed variation by shunt regulation depends on the reduction of the field strength, so that it is advantageous on this ground also to have the initial strength as high as possible.

It will be seen that the value of the core loss is not susceptible of any very great variation, that a reduction in the quantity of material with consequent increase in the induction does not materially affect the loss in the core. We therefore turn to A_{II} and A_{III} as the quantities susceptible of the most effective change.

A_{II} and A_{III} are seen to depend upon the weight of the teeth. Here again it is convenient to derive this weight directly from the slot depth, and a formula has been arranged and given in Appendix I., deriving the dimensions of tooth and slot from the slot depth, the induction in the air space and the maximum induction in the teeth. The factor which is susceptible of the most effective variation is the slot depth, and diagrams showing the effect of the variation of the slot depth take the form shown in Fig. 3, which is worked out for a machine of ten inches diameter.

The value of the induction in the teeth, though susceptible of considerable variation, cannot be reduced so as to materially affect the result. Considerations of commutation call for a high value of the tooth induction as well as of the induction in the air space, so as to provide a "stiff" field and to throttle or minimise cross induction and consequent shifting of the diameter of commutation with change of load.

The limit to the tooth induction appears to be fixed by the risk of the leakage field into the slot, causing eddy currents in the conductor. The values of the induction in the air space which appear to be adopted in current practice, and as indicated in the paper by Professor Silvanus Thompson already referred to, appear to be not very far from the value chosen in the Congress paper, viz., about 6,000 to 8,500 lines per centimetre.

The value of the tooth induction commonly adopted is a maximum of 20,000 to 22,000 lines per centimetre.

Speaking generally, the changes which can be made have the following effect :—

Increase of core induction, slot depth, air space induction, and tooth induction all have the same effect upon the output curve of the machine as increase in speed, and increase in slot depth produces by far the most conspicuous change.

The iron losses decreasing with decrease in slot depth, a point is reached when the slot becomes too shallow to hold the wire, and a limit is reached in this direction.

If the wire be outside the slot, the risk of eddy current loss must be taken into account.

Turning now to the value A $\sqrt{n}$. The radiation factor is susceptible of some variation. The demand for enclosed motors—which we may say, in passing, does not seem to have any good theoretical justification, but like so many demands which have become common,

and which have been accepted by the designers of machines without sufficient consideration or resistance to outside influence, appears to be entirely injurious—this requirement of enclosure of the motor, which prevents circulation of air, and thus prevents the armature from getting rid of its heat to the moving air, restricts the output of the machine quite unnecessarily. Under certain circumstances it is necessary to protect the motor from moisture, but the most injurious kind of dust which can lodge on an armature is the dust from the commutator and brushes. A good hard commutator and high quality of carbon brush make very little dust, but a certain amount of wear on the carbon brush is unavoidable, and the carbon dust is a conductor, whereas most of the dust floating in the atmosphere consists of oxides, silicates, and other non-conducting matter.

Artificial methods of ventilating the motor shell are possible, and in cases where the motor must be protected from damp may be found useful, but the best way of procuring a circulation of air is, where it is possible, to expose the motor freely to the atmosphere. Ventilation of the cores of small machines is an expensive, and on very small machines an impracticable, expedient.

Turning now to the question of efficiency—

This may be considered from the points of view of use of material, and of conversion of energy.

Taking the use of material first.

If we insert the value now given for W, the total watts of the machine, in the value of K (see Appendix III.), we get—

$$K = \text{ergs per second per cubic centimetre per second} \times 10^7$$
$$= \frac{\sqrt{A_{III}\, Q\, S_v} \times 10^{-5}}{\pi\, d\, s \times \sqrt{2L + \dfrac{D\, 7{\cdot}44}{P}}},$$

which brings out very clearly the importance of the cross-sectional area of the copper on the machine.

$Q\,S_v$ is this factor.

The energy per cubic centimetre of the active belt is proportional to the square root of this value, and inversely proportional to the square root of the length of each turn and to the depth of the slot.

The author has for some time been using the method of consideration first published, so far as he is aware, by Mr. Henry M. Hobart in his paper in *Traction and Transmission* in October, 1902, in which he lays great stress upon the importance of a high value for the cross-sectional area of copper in the active belt ($Q\,S_v$).

While admitting to the full the truth of Mr. Hobart's remarks on this point, it must not be forgotten that the insulation of armature conductors has to be considered as a question of mechanical clearance and protection as well as of specific insulation, so that it is conceivable that a high value of Q would result in greater risk of breakdown from the armature conductors coming into contact with one another in handling. This is a point which can only be settled by experience, and the arguments used in favour of multiple coverings on cables apply with *still greater* force to armatures, *i.e.*, that a covering in

many layers of a lower specific insulation is better than a covering of one layer of higher specific insulation. This accounts for the practice of covering armature conductors with several layers of tape, and, in addition, insulating the slot itself by means of paper, or binding paper to the conductors themselves. The use of a comparatively thick layer of paper is much more effective and safer than of a thinner layer of mica, which, though of a much higher insulating property is much more liable to mechanical damage.

The efficiency of conversion of energy is the ratio between—

$$\frac{\text{Total watts generated less watts lost in machine}}{\text{Total watts generated plus watts lost in iron}} = \frac{W - A_{\mathrm{IIII}}}{W + A_{\mathrm{V}}\sqrt{n} - A_{\mathrm{IIII}}}$$

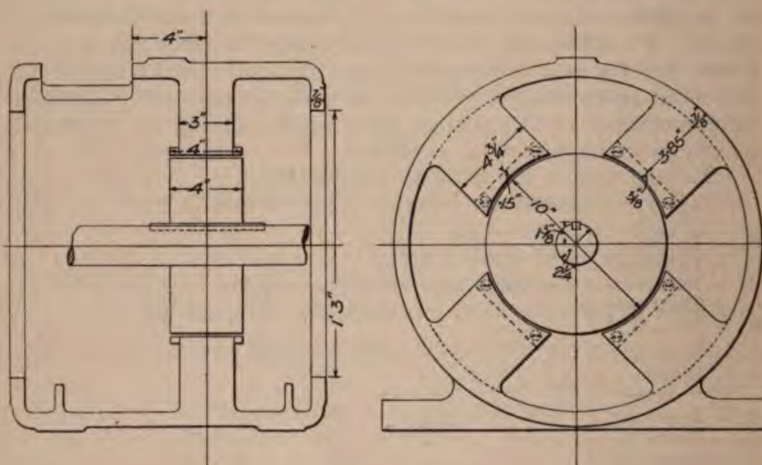

FIG. 4.

Here again the importance of keeping down the iron loss is clearly brought out, and the diagrams show the great importance of this factor in a small machine.

Figs. 1, 2, and 3 summarise the argument of this paper. They contain five curves, and have reference to a machine the carcase of which is shown in longitudinal and in cross section in Fig. 4. Curve No. 1 (Fig. 3) represents the total watts generated in the armature, which is 25·4 centimetres in diameter, 10·16 centimetres long, with a slot depth of about 2·38 centimetres, and was run at a speed of 1,000 revs. per minute, giving an output of 4,600 watts, with a temperature rise of about 70° Fahr. above the temperature of the surrounding air.

Curve No. 2 represents the total watts generated by a similar armature with a slot 1·6 centimetres deep, which was run at the same speed and gave approximately the same output.

Curves Nos. 1A and 2A represent the calculated outputs of two armatures, respectively with the same windings as 1 and 2 but with *no slots, i.e.*, smooth cores with different depths of winding.

Curve No. 3 is the approximate envelope of all possible curves of output for this carcase. This curve may be slightly varied by variation in the inductions, but within practical limits it is not subject to much variation, and shows the maximum output obtainable from the carcase at all speeds.

The magnet winding and air space induction are the same in all the cases.

A study of this diagram will throw considerable light on the problem of estimating the possible output from any carcase, and also upon the probable effect of speed of rotation on the output of any given machine carcase. It will be evident that as the iron losses become less important, as they do in the larger sizes, the envelope will tend to become fuller in form, and as the speed of the larger sizes of machines is limited by the strength of the material, the rate of rotation is reduced, the upper part of the curve becomes of less importance, and the lower part of the curve approaches more and more nearly to the straight line, which is consistent with the experience that on the larger sizes the watts generated are nearly directly proportional to the speed of rotation.

SYMBOLS USED IN APPENDICES.

B_a = Induction in air space in c.g.s. lines per centimetre.
B_c = Induction in core, lines per square centimetre.
B_t = Induction in teeth in c.g.s. lines per centimetre.
C = Total current in amperes.
E = Total E.M.F. of armature in volts.
G = Sections in commutator.
d = Diameter of core measured to the middle of the active belt, in centimetres.
d_1 = Internal diameter of core discs in centimetres.
d_2 = Diameter of core to bottom of teeth, in centimetres.
D = Diameter of core overall, in centimetres.
L = Length of core in centimetres.
m = Armature turns per section of commutator.
n = Revolutions per second.
p = Paths through armature.
P = Number of poles in the machine.
$Q = \dfrac{\text{Copper area in slot}}{\text{Area of slot}}$.
S = Depth of slot in centimetres.
S_v = Area in centimetres of 1 slot × number of slots.
$x = \dfrac{\text{Pole surface} \times P}{\pi D L}$.
y = Ratio of $\dfrac{\text{Iron in core}}{\text{total cubic content of core}}$.

APPENDIX I.

Formulæ and constants for determining core and tooth losses :—

Hysteresis and Eddies in the Core.

$A_1 n$ = weight of core discs in lbs. × $B_c^{1.6}$ × periodicity × $3{\cdot}25 \times 10^{-8}$.

Weight of core discs = $\cdot 0168\, y \frac{\pi}{4} (d_2^2 - d_1^2) \cdot L$.

Weight of one cubic c.m. of iron = ·0168 lbs.

$$B_c = \frac{B_a x \pi D L}{P y (d_2 - d_1) L}.$$

$$\text{Periodicity} = \frac{P n}{2}.$$

Hysteresis in Teeth.

$A_{II}\, n$ = weight of teeth $\times B_t^{1 \cdot 6} \times$ periodicity $\times 1 \cdot 85 \times 10^{-8}$.

Eddies in Teeth.

$A_{III}\, n$ = weight of teeth $\times B_t^2 \times \left(\frac{P n}{2}\right)^2 \times 3 \cdot 84 \times 10^{-12}$.

Weight of teeth is derived from—

Belt volume $= \pi \left(\frac{D}{2}\right)^2 - \pi \left(\frac{D - 2 S}{2}\right)^2 = \pi S (D - S)$.

Total width of teeth at bottom $= \frac{B_a x \pi D}{B_t y x} = \frac{\pi D B_a}{B_t y}$.

Total width of slots $= \pi \left(D - 2 s - \frac{B_a D}{B_t y}\right)$.

Volume of slots $= S_v\, L = L \pi s \left(D - 2 s - \frac{B_a D}{B_t y}\right)$.

Vol. of teeth $= L \left[\pi s (D - S) - \left(D - 2 s - \frac{B_a D}{B_t y}\right)\right] = L \pi s \left(S + \frac{B_a D}{B_t y}\right)$.

Weight of teeth $= \cdot 0168\, y L \pi s \left(S + \frac{B_a D}{B_t y}\right)$.

APPENDIX II.

These constants are believed to be reasonably accurate, but they have to be carefully checked by actual results for various types of carcase.

Formulæ and constants for determining radiation from the surface of the core only for a rise of 70° Fahrenheit, above an atmospheric temperature of 70° Fahrenheit :—

Partially enclosed Armatures, without ventilating spaces.

$A_v \sqrt{n}$ = watts radiated from core surface $= \pi D L \times \cdot 0073 \times \sqrt{\pi D n}$.

Open Machines, Armatures, with ventilating spaces.

$A_v \sqrt{n} = \pi D L \times \cdot 0152 \times \sqrt{\pi D n}$.

Open Machines, Armatures, without ventilating spaces.

$A_v \sqrt{n} = \pi D L \times \cdot 01 \times \sqrt{\pi D n}$.

Totally enclosed Machines.

$A_v \sqrt{n} = \pi D L \times \cdot 0073 \times \sqrt{\pi D n}$ for 90° rise.

Formulæ and constants for determining radiation from the whole surface of core and end winding for a rise of 70° Fahrenheit, above an atmospheric temperature of 70° Fahrenheit :—

Partially enclosed Armatures without ventilating spaces.

$$A_v \sqrt{n} = \pi D \left(L + \frac{2D}{P}\right) \times \cdot 00565 \sqrt{\pi D n}.$$

Open Machines, Armatures, with ventilating spaces.

$$A_v \sqrt{n} = \pi D \left(L + \frac{2D}{P}\right) \times \cdot 01 \sqrt{\pi D n}.$$

Open Machines, Armatures, without ventilating spaces.

$$A_v \sqrt{n} = \pi D \left(L + \frac{2D}{P}\right) \times \cdot 008 \sqrt{\pi D n}.$$

Totally enclosed Machines.

$$A_v \sqrt{n} = \pi D \left(L + \frac{2D}{P}\right) \times \cdot 00565 \sqrt{\pi D n}, \text{ for } 90^\circ \text{ rise.}$$

APPENDIX III.

Formulæ and constants for determining winding data :—

$$\text{Length of end winding} = \frac{4D}{P}\sqrt{1 + \frac{\pi^2}{4}} = \frac{D\,7\cdot 44}{P}.$$

$$\text{Length of one turn complete} = 2L + \frac{D\,7\cdot 44}{P}.$$

$$\text{Resistance of one turn} = \left(2L + \frac{D\,7\cdot 44}{P}\right)\frac{2mG}{QS_v} \times \cdot 2 \times 10^{-5}.$$

$$\frac{\text{Copper area}}{\text{Slot area}} = Q, \text{ area of each conductor} = \frac{QS_v}{2mG}.$$

Specific resistance of copper at 130° F taken at $\cdot 2 \times 10^{-5}$.

$$\text{Weight of copper on armature} = \frac{QS_v}{2}\left(2L + \frac{D\,7\cdot 44}{P}\right) \cdot 0194.$$

$$^{*} A_{IIII} = \left.\begin{array}{l}\text{watts lost in}\\ \text{winding}\end{array}\right\} = \left(\frac{C}{p}\right)^2 \left(2L + \frac{D\,7\cdot 44}{P}\right)\frac{2mG}{QS_v} \times \cdot 2 \times 10^{-5} \times mG.$$

$$\therefore \frac{GmC}{p} = \frac{\sqrt{A_{IIII}} \times \sqrt{QS_v} \times 500}{\sqrt{2L + \frac{D\,7\cdot 44}{P}}},$$

$$\text{and } EC = \frac{GmC}{p} \times \pi D L \times B_a x \times 10^{-8} \times 2 \times n,$$

* Where the output is calculated from the radiation from core only, substitute $2L$ for $2L + \frac{D\,7\cdot 44}{P}$ in value of A_{IIII} for watts lost in slot winding only.

Weight of one cubic c.m. of iron = ·0168 lbs.

$$B_t = \frac{B_a x \pi D L}{P y (d_2 - d_1) L}.$$

$$\text{Periodicity} = \frac{P n}{2}.$$

Hysteresis in Tee

$A_{II}\, n$ = weight of teeth × $B_t^{1.6}$ × periodi

Eddies in T

$A_{III}\, n$ = weight of teeth × B_t^2 × $\left(P\right.$

Weight of teet

Belt volume = $\pi \left(\frac{D}{2}\right)^2 - \pi \left(D - \right.$

Total width of teeth at bott

Total width of slots = $\pi \left(\right.$

Volume of slots = S_r L

Vol. of teeth = L $\left[\pi \right.$

Weight of teeth =

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er cm³ at unit velocity in field of unit
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refer to the matter again here ?

ant of work) are	...	$M L^2 T^{-2}$
ocity	... ...	$L T^{-1}$
ensity of magnetic field	...	$M^{\frac{1}{2}} L^{-\frac{1}{2}} T^{-1}$
rrent (electro-magnetic)	...	$M^{\frac{1}{2}} L^{\frac{1}{2}} T^{-1}$

er per unit velocity in field of unit intensity
imensions—

$$(L^{-1} T)\ (M^{-\frac{1}{2}} L^{\frac{1}{2}} T) = M^{\frac{1}{2}} L^{-\frac{3}{2}} T^{-1}.$$

$$M^{\frac{1}{2}} L^{\frac{1}{2}} T^{-1})\ (L^{-2}) = M^{\frac{1}{2}} L^{-\frac{3}{2}} T^{-1}.$$

another light—One conductor on the periphery
g in field of unit intensity at unit velocity has unit
er unit length. If L be the length of the con-
the total number of conductors on the arma-
s symbols), if A be the area per wire and δ the
the power manifested in the active belt will be
olume of copper in active belt.

wer in active belt, we may write the watts manifested
of active belt

is proportional to $\delta \gamma$ × volume of active belt. Hence
that $\delta \gamma$ is practically a constant for all dynamos.
equation for W on page 476. I puzzled for a consider-
I saw the meaning of this equation, probably because I

was too lazy to read through Appendix III. In any case, this formula does not agree with that in Appendix III. This formula simply says $W = E\,C$ in a rather involved manner. $W =$ current per turn × E.M.F. per turn × number of turns, and this is independent of whether the armature is series or parallel wound. If $C =$ current per turn, $R =$ res. per turn. $C^2 R\,m\,G = A_{IIII}$, or $C = \sqrt{A_{IIII}} \,/\, \sqrt{R\,m\,G}$. Mr. Field.

$$\sqrt{R\,m\,G} = \sqrt{\left(2\,L + \frac{D\;7{\cdot}44}{P}\right)\frac{m^2 G^2}{Q\,S_v}\,2 \times 10^{-3}}$$

and E.M.F. per turn $= 2 \times \pi\,D\,L\,B_a\,x\,n\,10^{-8}$.

$$\therefore\; W = \frac{\sqrt{A_{IIII}}\,\sqrt{Q\,S_v} \times \pi\,D\,L\,B_a\,x\,n\,10^{-5}}{\sqrt{2\,L + \frac{D 7{\cdot}44}{P}}}.$$

I think the usual procedure in designing a dynamo is to fix on a current density (determined by previous experience), and by trial and error determine the number of turns per coil and wires per slot, this being to a large extent settled by the permissible reactance voltage per slot, and from that basis to arrive at the number and shape of slots. Mr. Mavor rather indicates the reverse procedure; he considers a given carcase—at each speed only a definite number of watts may be dissipated in the armature, provided the temperature rise of the same is limited. Now, determine the total iron losses at the particular speed, deduct these from the total permissible loss, and the result is the permissible copper loss. This settles then the output of the generator. I do not think that this can form a good basis of calculation, because one cannot calculate a machine from one point of view alone. The best machine from one point of view may be the worst from others; the design of such a machine is always a compromise between different conflicting requirements. We must consider at the same time heating of armature, due to itself and due to the neighbourhood of the field coils, the reactance voltage, total reaction, total copper on field magnet, mechanical considerations, etc., etc. All these considerations are inextricably entangled, as stated, one cannot design a machine from one point of view alone; it is always a case of compromise.

It seems to me that in designing a small machine the dimensions of the slot to a large extent settle themselves automatically. The turns per coil cannot be very smoothly graduated; we shall have, say, 4, 6, or 8. We prefer to choose a number which has convenient submultiples, so that the windings may be put in parallel for submultiples of the voltage. If 4 turns were unsuitable, we should take a jump straight away of 50 per cent., and go up to 6 turns, or of 100 per cent., and go to 8 turns. Which of these coils we adopt will be largely determined by the reactance voltage allowed (*i.e.*, commutation troubles). The arrangement of the wires in the slot will be largely settled by the convenience of winding the coils; the wire is round, and thus the dimensions of the slot gradually take shape. I do not say that no choice is left; it is possible to group more or less coils in one slot, and then to vary the number of turns per coil and number of slots; but there are, as a rule, not many alternatives open; we can only go in jumps, and it is usually either one thing or the other.

Mr. Field.

Mr. Mavor draws attention to the question of space factor. It is obvious that if we can get 5 per cent. more area of copper on a given armature without altering the iron dimensions, we can take 2½ per cent. more load from the machine with the same temperature rise. The space factor, or what Mr. Mavor has called Q, is a very important one. By grouping the coils together in one slot we naturally save room in insulation and increase this factor.

The old line of traction motors designed by the G. E. Co. compared with the newer types are interesting—

OLD LINE.			NEW LINE.		
G.E. 800	27 H.P.	105 slots.	G.E. 52	27 H.P.	29 slots.
G.E. 1000	35 H.P.	93 „	G.E. 58	37 H.P.	33 „
			G.E. 57	37 H.P.	33 „
			G.E. 51	80 H.P.	37 „
			G.E. 67	40 H.P.	51 „

I want now to say a few words *re* the curves Mr. Mavor has published. He tells us, in the first place, that the heat which can be radiated from an armature is directly proportional to $\sqrt{n}$, and in his curves he has apparently applied this empirical formula throughout the whole range of speed; in fact, down to $n = 0$. Of course, if $n = 0$, one can still absorb quite an appreciable amount of power in the armature without the temperature rising above the given limit (say, 70° Fahr. rise). I would suggest that this formula be more accurately expressed as $a + \beta\sqrt{n}$, a being the permissible loss if the armature be stationary, $\beta\sqrt{n}$ the permissible excess loss due to the cooling effects of the rotation. Again, Mr. Mavor takes the hysteresis and eddy current loss in the body of the core, as $A_{I}\,n$, and in the teeth, as $A_{II}\,n + A_{III}\,n^2$. Eddy current loss is always proportional to the square, and hysteresis to the first power of the periodicity. Now, the eddy current loss is proportional to the weight of iron and the square of the induction; hence it seems to me that the eddy loss in body of the core is equally important as that in the teeth, unless anything special occurs in the teeth. If the formulæ given by Mr. Mavor really agree with practice, it seems as if something special in the way of eddy currents must occur in the teeth, and I suggest that this may occur in their top and side surfaces. Perhaps Mr. Mavor will give us further information on this point. Does he turn the outside periphery, file the slots, and afterwards pull the plates apart, take off the burrs, and anneal the plates? Or what is the procedure?

With regard to the curves of maximum output, it is evident that if the armature be open-circuited and the speed of the generator be gradually increased, a point will be reached where the iron loss alone equals the permissible armature loss, and this will be the upper limit of speed, the output being zero. This speed is determined by the equation: $a + \beta\sqrt{n} = \mathrm{A}\,n + \mathrm{B}\,n^2$, provided the permissible loss can be expressed by the expression on the left, and that on the right represents total iron loss.

Mr. Field

Again, if the armature be short-circuited at a certain speed, very nearly zero, the C^2R loss will be equal to a, and this may be taken as the lower limit, the output again being zero. Between these limits the output rises to a maximum, and then decreases, as in Fig. 4.

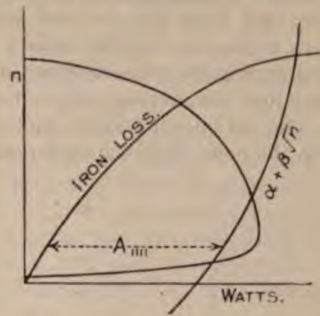


FIG. 4.

But it does not pass through zero. The iron loss and radiation curves should again be as I have shown, not as in Mr. Mavor's diagram.

Lastly, the question of enclosed motors. I am very glad this has come up. Two or three years ago some of the leading firms were standardising enclosed motors. What they did was this. They took, say, a 20 H.P. open motor, and rated it as a 15 or 10 H.P. enclosed, without any attempt to modify the design This, of course, was absurd, as the proportion of iron to copper losses could not be the best in each case.

If $V\,C$ = input of motor,
σ = fixed losses due to iron, friction, excitation, etc.,
$\tau\,C^2$ = variable losses,
η = efficiency,

$$\text{Then } \eta = \frac{V\,C - \sigma - \tau\,C^2}{V\,C} = 1 - \frac{\sigma}{V\,C} - \frac{\tau\,C}{V}$$

$$\text{and } \frac{d\,\eta}{d\,C} = \frac{\sigma}{V\,C^2} - \frac{\tau}{V}.$$

The efficiency will be a maximum when $\frac{d\,\eta}{d\,C} = 0$, or when $\sigma = \tau\,C^2$, or the fixed and variable losses are equal in amount. This, of course, is well known.

Now, if a 20 H.P. motor be designed for highest efficiency at full load, it will be manifestly very poor policy to turn it into an enclosed motor and rate it down. By re-designing it, decreasing the iron losses, and increasing the C^2R, we could get a much better efficiency as an enclosed motor. If you enclose a motor you must re-design it. In the same way, if one designs a transformer for natural cooling, then applies the air blast and rates it up, one does not get the best result: it should be re-designed. This whole problem is very interesting. If one wants to sell a machine cheap one should make the maximum efficiency at full load, because the full load loss will be smaller than if the maximum efficiency occurred at any other load, and hence for given heating full load can be rated high. It does not follow that this will be the best motor from the consumer's point of view; it may have to run light for a large portion of the time, in which case it would have been better to have a motor which had smaller fixed losses, or maximum efficiency at something less than full load. In such a case the heating effect at full load would be higher, and if the same specification were abided by

Mr. Field. it would mean a larger motor; but the consumer's current bill would be less.

Mr. Ker. Mr. W. ARTHUR KER, Assoc. M. Inst. C.E., said that Mr. Field had put forward the opinion that for a maker of an open-type motor to put covers upon it and call it an enclosed motor at a lower rating was not good practice. Mr. Field said that the designer should endeavour, for maximum efficiency of a commercial machine, to make eddy current and hysteresis losses approximately equal to the copper losses; but by enclosing an open machine, and running at a lower amperage, the copper losses were considerably reduced, while the core losses remained as before, and therefore on light loads the machine was very inefficient. The speaker stated in reference to this that it was the practice of his company and several others to enclose their ordinary open-type machine, and run it at a lower rating and at a lower speed. This was the simplest way of obtaining the result desired by Mr. Field. At the first glance one would say that by doing this one would reduce the core losses slightly on account of the lower speed; but that the reduction would not be comparable to that of the copper loss due to the lower rating. The benefit of the lower speed, however, is much more than this. The output of a machine, except in very small sizes, is usually limited by the reactance voltage. By reducing the speed from, say, 1,000 revolutions as an open motor to 900 revolutions as an enclosed motor, the periodicity is reduced sufficiently to permit (taking account of the smaller current) an addition of at least one armature turn to each commutator section, with the same reactance voltage. This means that the total length of copper on the armature is increased, while the cross section is reduced, and the copper watts become approximately equal to the core losses. This appears to be quite the simplest method of meeting the case, and from tests of a large number of machines made, both open and enclosed, using the same carcases, the efficiency of the enclosed machines is within 2 per cent. of that of the open type. This is a very small penalty to pay for the advantage of having a machine which is protected by solid iron cases from any chance of injury due to falling objects, damp, and dust. In a well-designed motor ample access is given to the commutator and brushes in order to remove the carbon and copper dust which in the course of time accumulate upon them. The speaker therefore cannot agree with the author in his condemnation of the enclosed machine. He is of opinion, however, that the enclosed ventilated motor, which has lately become fashionable, is most objectionable. If it really complies with its name and ventilates efficiently it draws all the dust and moisture-laden air in the neighbourhood over its windings and commutator, the moisture being evaporated and the dust remaining behind, the latter being practically impossible to remove without dismantling the machine.

The author states that "it is customary to treat the eddy currents in the core as being proportional to the periodicity." This is not the speaker's experience, as he can see no reason for treating the eddy current losses in the core differently from those in the teeth. Usually these losses are very small, but not always. The speaker has recently

had the design of a machine through his hands running at 3,000 revolutions per minute in which the eddy currents were considerably in excess of the hysteresis losses. The author states that "the radiation of heat from the armature is taken as proportional to the square root of the speed, and gives a diagram showing the radiation curve starting from the origin at zero speed." This does not appear to be the case. The cooling of the armature is caused by (first) radiation, (second) convection air currents, (third) air currents caused by the fanning action of the core. When the machine is standing still a certain amount of heat is dissipated by radiation and convection currents, the curve should therefore begin at a point above the origin. What appears to be really the case is, that the heat dissipated is equal to a constant multiplied by an expression which is directly proportioned to the peripheral speed. Mr. Ker.

In Appendix II. the author gives a formula for the watts radiated from the core surface, which he states is equal to the surface of the core $\times$ a constant $\times$ square root of the peripheral speed; but this does not appear to be the case. In an ordinary centrifugal fan the air discharged is directly proportional to the peripheral speed, and an armature (especially if fitted with ventilating ducts) should follow the same natural law. In that case the heat dissipated by air currents should be proportional to the air discharged—that is, proportional to the peripheral speed. The speaker believes that this is true. He has used, for a considerable time, with very accurate results a formula put forward by Mr. Kapp. It is given by Mr. Kapp in the form of temperature rise for a given surface, watts dissipated, and peripheral speed, and has been altered to give watts dissipated and English units, and is as follows:—

Watts dissipated at 70° Fahr. temperature rise $= \cdot 00023 \times$ surface of core (1970 + peripheral speed in feet per minute).

It is, of course, empirical; but for small and moderate-sized machines has proved reliable; for large machines the rise in temperature is generally so small that the opportunity of verifying the formula in these cases has not been afforded. The meeting might be surprised at English units being used in dynamo calculations at the present day, but the speaker is of opinion that a judicious blending of English and c.g.s. units is of great assistance in getting out designs. He is in the habit of expressing all areas and volumes in centimetres, and all lengths in inches, the reason being that the ampere-turns per inch length in air is (very approximately) equal to twice the number of c.g.s. lines per square centimetre. By adopting this method, and having books printed giving the various formulæ required in their regular sequence, spaces being left for the insertion of figures and noting results, there is no reasonable chance of error, and a great deal of time is saved.

The author states that the tooth inductance commonly used is a maximum of 20,000 to 22,000 lines per square centimetre. That appears to be rather high, the ampere-turns required to force the flux through the teeth being excessive, and probably a density of 18,000 is more generally satisfactory. Many designers are abandoning the high *densities in the* teeth, as they are of opinion that a sufficiently

Mr. Ker. "stiff" field can be obtained without this. A low reactance voltage is evidently the most important requirement for sparkless commutation, and with the small polar angles which are taken by many designers at present (quite unnecessarily in the speaker's opinion) a very stiff field is not required. He had occasion the other day to make some tests on a motor which had a very "strong" armature, the cross ampere-turns under pole being equal to the ampere-turns on field required to force the flux through the air space. By means of a resistance in the shunt and by reducing the load the ampere-turns on the field were reduced to one-fifth of the normal, while the armature current was kept constant. Even under these conditions there was no sparking at the brushes, which had not been moved. The polar angle was 72 (4-pole motor), but why the machine was so insensitive the speaker could not say. The experiment, however, indicates that very heavy inductions are not necessary to ensure sparkless running if the reactance voltage is kept low (this was about 3½ volts at normal speed). The experiment was one of several on different machines, all with much the same result. In this connection he would be glad if the author would inform the meeting how he arrives at the area of the air space. Glancing through the formulæ in the appendices, the speaker cannot see that one included, though the density of the air space is used in the formula for obtaining the density in the core, and from the value of the symbol x (which is included in the same formula), the area of the air space appears to be taken as that of the poles. This cannot be the real air space area, and it would be of help if the author would give the formula he uses.

The speaker said that in the formula in Appendix III. for obtaining the length of end winding no allowance was made for different numbers of slots. As a matter of fact, the length necessary is very much affected by the number of slots and the ratio of slot width to width of tooth. In machines with two or more coils per slot (in which direction modern design for small machines, and the paper deals with small machines only, tends) the end winding can be much shorter than with machines having one coil per slot, as practically the same thickness of insulation serves for two coils in place of one, and the angle of the double helical-formed coils can be much acuter. The depth available for the outer ends of the coils also affects the length necessary, but no provision is made for this in the formula.

The author expressed the opinion that "ventilation of the cores of small machines is an expensive and, on very small machines, an impracticable expedient." This the speaker considered to be only partially correct. If the author is in the habit of milling the slots in the core, the ventilating ducts must prove a difficulty; but if the slots are notched in a stamping press the extra cost of stamping air ducts in the plates of small machines (in large machines the spider is provided with longitudinal ducts) is very slight, and, in fact, the stampers who supply the trade do not charge extra for this. The cost of distance pieces to keep the plates apart at the ducts is only a matter of a few pence per machine. The gain by having ducts is so great, increasing the cooling surface by an amount equal to the area of each side of each

duct, that the cost of the ducts is covered many times over by the extra output obtainable with the same temperature rise. The speaker uses air ducts in all armatures from 7-inch diameter upwards; under 7 inches diameter it is impossible to find space for the longitudinal ducts. He has listened with much pleasure to the paper, and the impression he has gained is that the trend of it is this, that if you have a fast running machine it is best to have a shallow slot, and keep the iron losses down; and this is possible, because a high speed requires few armature conductors, therefore less slot volume is required for copper and insulation. With a low speed a deep slot must be used, as many armature conductors and much insulation are required. Mr. Ker.

DISCUSSION CONTINUED, DECEMBER 9, 1902.

Mr. W. B. HIRD said that his name had been mentioned in connection with certain of the formulæ in the paper, more especially in connection with Mr. Field's remarks regarding them. There were two points which Mr. Field had criticised. The first was that the formula Watts radiated $= \alpha \sqrt{\eta}$ should be $\alpha + \beta \sqrt{\eta}$. Mr. Hird contended that both formulæ being empirical and the value of the constants having to be determined as the average of a number of experimental results, it was within ordinary limits of speed, say 1,000 to 300 revs., possible to find a value of α which in the first formula gave results quite as nearly in agreement as did any values for α and β in the second formula, whilst the simplifying of the equations was greatly in favour of the first formula. It was of course quite true that for speeds either above or below a certain limit the first formula did not represent the facts. Mr. Kerr wished to substitute "n" for the $\sqrt{n}$ in the formula $\alpha + \beta \sqrt{n}$. In Mr. Hird's experience this would give calculated values of radiation at high speeds much in excess of the truth. The second point raised was that eddies in the core should vary as n^2, not as n: certainly eddies vary as the square of the speed, but except at very high speeds the eddies in the core are unimportant compared to hysteresis and teeth losses, and therefore the error caused by treating the core eddies as a percentage of the core hysteresis is only small. On the other hand the high complexity of the flux distribution in the core makes it very difficult to estimate the value of the coefficient if the eddies are treated, as strictly speaking they should be, as equal to $\alpha \, W^t B^2 n^2$. Mr. Field pointed out that the ergs per cubic centimetre of active belt at unit speed and with unit magnetic flux is equal to current density per square centimetre of belt area, and said that it was, therefore, not remarkable that the ergs should work out approximately equal in different designs, as this only meant that different designers worked at about the same current density. It is, however, not the current density in the copper which must be kept the same, but the current density in the copper multiplied by Q, and it is somewhat remarkable that as the value of Q is altered the current density in the copper should alter in inverse ratio so as to keep the product constant. Mr. Hird.

But his chief object, however, was to point out that the value of the paper appeared to him to be quite independent of these questions. The

Mr. Hird curves drawn on the board and given in Fig. 2 seemed to him to be the important thing; the principle that this graphical method should be adopted for determining the best speed at which a machine should be run, or if the speed were fixed the best depth of slot to be used, is entirely independent of the particular form of formulæ used. Every designer must have some formulæ, some method of determining the iron losses and the watts radiated. If a designer preferred his own formulæ to those given in the paper, let him use them in the manner indicated, and instead of merely knowing in a vague way that if the speed gets very high the iron losses will equal the possible radiation and the permissible copper loss will become zero, and he will therefore get no output, let him plot the curves shown, using his own formulæ, and find out exactly at what speed this happens. It was, of course, perfectly true, as Mr. Field had pointed out, that this did not completely design the dynamo. It did not enable them to get a round wire into a square slot which it would not fit to any advantage, and they still had to begin by finding a suitable slot and wire to fit one another; but the curves were a valuable indication of the direction in which they should work in looking for a suitable slot.

Mr. McWhirter. Mr. Wm. McWhirter said it was refreshing to see that there was still something worth discussing in the design of C.C. dynamos. Some years ago a member of this Institution had stated that a cow of average intelligence was capable of designing such a machine, as it simply consisted of two bearings, a shaft and a pulley; this notwithstanding, we had since had many excellent papers on the subject, and although we had often heard that the C.C. dynamo or, in fact, any type of commutating machine ought to be relegated to the scrap heap, still the time for this relegation instead of being within sight, appeared to be receding into the distant future, and therefore every suggestion for the improvement of C.C. machines should be heartily welcomed.

Mr. Mavor said that in his Congress paper on dynamo designing he "suggested that the essential part of the dynamo is the region occupied by the armature conductors in the magnetic field." But had not this suggestion been before the Institution for many years? Almost every paper on dynamo designing during the last twenty years had been imbued with the idea, and many formulæ had been proposed giving the output of dynamos in terms of the dimensions of this part of the machine. In this paper no mention was made of any part of the dynamo outside the armature. The paper was unique in this respect. Another striking point was that the question of commutating was hardly referred to, and we ought all to rejoice that this subject has now been found to be quite unnecessary. This was certainly the greatest improvement made in recent years in dynamo construction. The speaker once had under his care one of the largest C.C. dynamos which had then been made, the brushes for which alone had cost over 10s. per week, whereas now we had similar machines running which did not cost so many pence. Most of the so-called inventions in connection with special windings and pole-pieces on dynamos were made not only to improve the commutation, but to give a position where the brushes might be absolutely fixed and practically independent of any change in

output. These proposals had not always been successful, and the large amount of energy, time, and money spent upon the attempts in this direction had certainly not been repaid. The simple adaptation, however, of the carbon brush properly applied had brought about more improvement in this respect than all the attempts referred to, and we ought to give more credit for the great improvement due to this simple innovation. Mr. Mavor's aim seemed to be a proposal for further improvement in the dynamo, thereby increasing the efficiency by reducing the losses set forth as items 1 to 6 on page 474. Generally speaking, there was not much difficulty in arriving at the temperature of the dynamo winding at any moment, by the simple plan of noting the falling-off in the shunt current (of course maintaining a constant voltage or correcting for variation), and this would certainly give a more reliable result than the application of thermometers to the outside of windings, etc. The temperature found by the increased resistance would approximately give the mean temperature, whereas the thermometer has usually to be applied to what is in reality the coolest part of the machine. The paper was evidently an attempt to settle the question of the best dimensions for the armature slots, or to put it otherwise, "deep" *versus* "shallow" slots. There certainly must be one dimension of slot which is the best for a certain output at a given speed and a given voltage, but the best slot dimensions must vary both with the voltage and the speed, as insulation of conductor and slots bear a large proportion to the copper section, and as the voltage increased more care was necessary to maintain the insulation between the conductors themselves and between the conductors and the core. He was not clear upon the curves given by Mr. Mavor, more especially Plate 1, and as the symbols used for the various losses would require so much time to check his figures properly, he had not had any inducement to look further into the matter. He wished it were possible for writers on dynamo-designing to use a common set of symbols, a thing which would make such papers far more easily intelligible. He also thought that Mr. Mavor might have spoken of revolutions per minute instead of per second. Mr. McWhirter.

Professor MAGNUS MACLEAN said that the paper by Mr. Mavor was one of great importance, and his contentions should be subjected to all the useful criticism designers of small dynamos and motors could bring to bear on them. For his own part he only wished to draw attention to one or two points that Mr. Mavor might elucidate in his reply. In the Appendix the hysteresis loss in the teeth was taken to be proportional to $B_t^{1.6}$, whereas eddy currents in the teeth were taken to be proportional to B_t^2. Why? It was, he thought, pretty well established by experiment that the hysteresis losses reached a maximum at a flux density of about 16,000 c.g.s. lines per square centimetre, and that for higher flux densities the hysteresis loss diminished asymptotically. Still further experiments were necessary to elucidate this subject. Mr. Mavor very pertinently remarked that the "story is not yet told of all that takes place in a piece of iron under changing magnetisation." He (Dr. Maclean) had had in view for some time to rotate a disc of copper and a disc of iron in a calorimeter placed between the poles of an Professor Maclean.

ofessor acIean. electro-magnet. If discs of different thicknesses were rotated at different constant speeds and in different constant strengths of fields, he believed very useful information could be obtained as to the hysteresis and eddy current losses that took place in the armature cores of dynamos and motors.

It was pointed out by Mr. Field that the constant, K, Mr. Mavor introduced, viz., ergs per second per unit volume at unit velocity in unit field, was equivalent to current per unit area. Mr. Field did so by substituting for each factor its dimensional expression in terms of length, mass, and time. He was a great believer in dimensional expressions, and he was of the firm opinion that electrical engineers did not devote the attention to that important subject that it deserved. But in the present case the equivalence could be arrived at quite simply.

$$K = \frac{\text{Activity}}{\text{Volume} \times \text{velocity} \times \text{flux}}$$
$$= \frac{\text{Current} \times \text{electromotive force}}{\text{Area} \times \text{length} \times \text{velocity} \times \text{flux}}$$
$$= \frac{\text{Current}}{\text{Area}}$$

:. Mavor. Mr. H. A. MAVOR, in reply, said that he wished to thank the members for the manner in which they had received his paper and discussed the points raised. The subject was one the interest in which was limited to a comparatively small number of the members, but it was none the less important on that account.

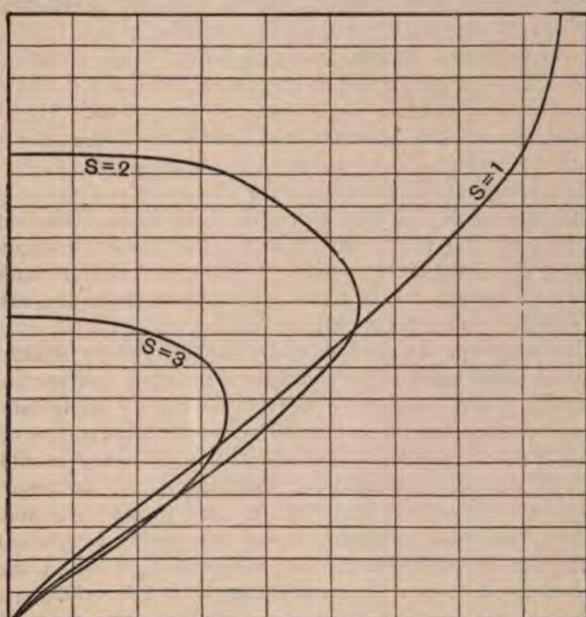
With regard to Mr. Field's remarks, he thanked Mr. Field for his contribution to the discussion and for the correction of certain typographical errors in the proof of the paper, and said that he had rightly understood the position adopted by the writer of the paper, viz., that it was necessary in small machines to consider the speed at which the machine had to be run with special reference to the iron losses. Mr. Field stated that the usual procedure in designing the dynamo is to fix on a current density determined by previous experience, and by trial and error determine the number of turns per coil and wires per slot, this being to a large extent settled by the permissible reactance voltage per slot, and from that basis to arrive at the number and shape of the slots. The writer of the paper agreed with Mr. Field that it is impossible to design a machine from one point of view alone, and that the design is always a compromise between different conflicting requirements. He argued, however, that if you begin by requiring an impossibility from the machine, there was not much use in going into other details. His method was to plot out the watts radiated at each speed with the iron losses, and to deduce the curves described in the paper, assuming, for preliminary purposes, unity as the value of the ratio between copper area and slot area—that is to say, assuming that the copper fills the slot.

Reference to the formulæ in Appendix III. would show that the watts output derived from this assumption must be multiplied by the square root of the actual value of this ratio to obtain the actual output of the machine. This ratio is determined by the considerations

Mr. Mavor.

mentioned by Mr. Field, viz., the consideration of reactance voltage and other practical requirements, but the slot width being determined by the length of the air space, and the necessity for avoiding heating of the pole-tips by eddy currents, the best slot depth is ascertained from the curves plotted as described in the paper, and it will be seen that there is not much room for choice in the other points which require to be considered, so that the actual output of the machine is fixed at a maximum which it is the aim of the designer to obtain as nearly as possible.

Mr. Field's reading of the value of the constant used for energy generated in the active belt is quite correct, but it is not in any sense



at variance with any of the arguments of the paper. It is only another way of saying the same thing. It is necessary to point out that the assumption of value for current density is the conductor in a first approximation of the design of the machine, exactly gives away the whole case. This quantity is among the very last to be determined. The current density in the conductors is of comparatively little importance in small machines; in fact it will be seen from the efficiency formula that the efficiency of the machine may be reduced by reducing the current density in the conductors.

Mr. Field's point with regard to the watts radiated is worthy of attention, and it would probably be better to plot the curves as he suggested. On the other hand, the curve $\sqrt{n} \times$ a constant, being derived entirely from experimental results, applies only to the range of

Mr. Mavor. these records, and this is from $n=8$ to $n=20$, or thereby. It can be extended to wider limits if need be, but for ordinary standard machines it is not necessary to calculate the curve below $n=8$, and properly speaking it ought not to have been drawn below this point.

The line of argument of the paper is that there are several speeds at which a machine carcase will give the same output, and that one of these speeds is the best. Approximate curves of the outputs obtainable from a 10-inch machine were shown, and are added to the Appendix of the paper. It was pointed out that in the case of the machine in question, the curves of which are plotted for an assumed unit value of Q, the space factor, the output of a machine wound with a two-centimetre slot was very different from a three-centimetre slot at the assumed speed of $n=16$ per second, and that if any circumstance arose to increase the temperature rise, either from variation of the quality of the iron in the ore or otherwise, the use of a three-centimetre slot on this machine would be very dangerous. The use of a two-centimetre slot, on the other hand, gives an increased margin of output in the ratio of 6 to 10, as indicated by the points marked A and B respectively on the curves for the three-centimetre and the two-centimetre slots. The envelope of the curves drawn for each slot depth gives the maximum value for the output of the machine, and having determined the speed at which it is to run, the slot which gives the best output at that speed without any tendency to fall away from a straight line on the curve, for the slot chosen is the best slot to use for the machine. In the case under consideration a two-centimetre slot would be approximately the best for a speed of $n=16$.

Replying to Mr. W. Arthur Ker's remarks, the arguments of this paper are commended to the further consideration of the members, because if they are borne out by the facts they show that each machine ought to be differently wound for each speed and output, and that a simple reduction of the speed of an open motor so as to obtain a satisfactory enclosed machine is not the right way to solve the problem. On the other hand, the line of design indicated by Mr. Ker's remarks are quite in accordance with the ordinary modern practice, but the speaker's contention was that this practice is not sound.

The author thanked Mr. Hird for his defence of the formulæ used in the Appendix, but pointed out that the correctness or otherwise of these formulæ had nothing to do with the argument of the paper, his contention being that it was necessary to examine all formulæ for determining the iron losses on the lines indicated in the paper, so as to obtain a comprehensive grasp of the conditions and avoid elaborate calculations with regard to the possible windings for a machine which left out of account altogether the principal factor of primary importance in laying out the design. The author agreed with Mr. McWhirter in the continued importance of continuous-current machinery, but pointed out that his paper had reference to small machines only.

He pointed out that the long expression referred to by Dr. Maclean

was not used for a calculation of the area of each conductor, but was a symbolic expression of the quantities on which this area depends. The formulæ given for this purpose will be found convenient in attacking dynamo design from this point of view. The author claimed for his paper that it was not a treatise on the whole subject and did not take up all the points necessary to be considered, but he wished further to commend a study of the subject on the lines indicated, having no desire to magnify the results already obtained, but ventured to think that such a study would lead to very considerable modifications along the line of improvement in the design of small machines. Mr. Mav

GENERAL RULES FOR WIRING FOR THE UTILISATION OF ELECTRICAL ENERGY.

1. These rules embody the requirements and precautions which the Institution has framed to secure satisfactory results with supply at a pressure not exceeding 500 volts if continuous or 250 volts if alternating. They are intended to include only such requirements and precautions as are generally necessary, but they are neither intended to take the place of a detailed specification, nor to instruct untrained persons.

2. Notice of the proposed introduction of wiring should in all cases be given to the Fire Offices insuring the risk, and to the suppliers of the electrical energy if such is to be obtained from an external source.

GENERAL ARRANGEMENT.

3. Conductors must radiate from distributing centres, and in large systems from those centres to sub-centres, so that no sub-circuit carries more than 5 amperes up to 125 volts, or more than 3 amperes from 125 to 250 volts, for incandescent lighting.

4. When protected from mechanical injury by hard metal tubes or conduits, conductors even of opposite polarity may be "bunched," and when carrying small currents from sub-centres, as in paragraph 3, they may, if without joints, be "bunched" even when the protecting tubing or casing is non-metallic. If the supply is alternating and the protection metallic, conductors must be bunched so that the sum of the currents passing is zero.

5. When one of the main conductors of a system of supply is earthed, no interruption of the current by any mechanical device is permitted in a conductor connected to the earthed main that does not also, and simultaneously, break circuit on the non-earthed conductor. Hence, to insure the current being interrupted simultaneously on both the earthed and the non-earthed wires, no switch that is not linked to another switch on

the non-earthed conductor may be inserted in any conductor connected to an earthed main.

6. No fuse may be placed in the neutral conductor of a "three-wire" system. This does not prevent the use of a disconnecting link in the neutral for testing purposes, but fuses must be placed on both conductors of two-wire circuits branching therefrom.

7. Every system must be controlled by linked main switches, which must be placed as near to the entry of supply to a building as circumstances permit, and which must be easily accessible. Subject to paragraph 6, the system must also be protected by main fuses.

8. Every sub-circuit must be protected on both poles by a fuse; and no single-pole switch may be inserted in the earthed side of a system.

9. When the wiring is such that one conductor is uninsulated at all points—such as a bare return to a concentric system—no switch or fuse may be placed in that conductor, and the said conductor must be efficiently earthed.

10. When the supply is from all three conductors of a *three-phase* system, each conductor must be protected by a fuse and the whole controlled by three linked switches.

11. When the pressure between outer conductors of a three-wire main exceeds 250 volts, the circuits connected to opposite sides of the neutral conductor must be so disposed that a person cannot simultaneously touch two points respectively in contact with the outer conductors.

12. Conductors conveying currents at pressures exceeding 250 volts must be completely enclosed in strong metallic sheathing or tubing efficiently connected to earth, and such sheathing or tubing must be electrically continuous throughout its length.

13. No switch, cut-out, connector, or other electrical appliance, may be mounted directly upon any surface of a condensing or humid nature, such as masonry, brickwork, cement, or plaster—but must, in addition to its own mount, be fixed upon a base block rendered impervious to moisture.

14. Branch fuses must be grouped together in accessible positions in sight, and should be symmetrically placed and labelled for each circuit.

15. Contact between insulated conductors and gas-pipes, or metals in contact therewith, must be prevented by non-conducting incombustible distance-pieces.

16. Gas-pipes must never be used to obtain an earth connection.

17. Switches and fuses, not in an engine-room or compartment specially arranged for the purpose, must be covered.

CONDUCTORS—CONDUCTIVITY AND SIZE.

18. The sectional area of conductors (see Table) must be greater than that determined by the heating effect of the current required for the maximum number of lamps, or other current-using apparatus, that can be used simultaneously on the circuit.

19. The size of conductors within a building will, subject to paragraph 18, be determined by the permissible drop in volts, which should not exceed 2 per cent. on lighting circuits.

20. Copper conductors should be of soft copper, and should have a conductivity not less than 100 per cent. as compared with Matthiessen's[1] standard; and where sulphur compounds are present in any part of the insulation the copper in contact with the insulation must be protected therefrom by tinning or otherwise.

21. The sectional area of a copper conductor must not be less than that of No. 18 S.W.G. wire, with the exception of the case of flexible cord conductors and wires for fittings, when the sectional area must not be less than that of a No. 20 S.W.G. wire. All insulated copper conductors having a greater area than that of a No. 14 S.W.G. wire must be stranded.

22. The table appended shows the sizes of copper conductors which will safely carry currents up to 740 amperes, and the length in yards of single conductor in circuit for each volt of fall of potential when the maximum current is in use.

CONDUCTORS—INSULATION.

23. Conductors must be specially insulated with material which does not deteriorate at the highest temperature to which it will be subjected; for instance, rubber must not be allowed to exceed 130° F., or paper—or fibre—insulation 170° F. In specially hot places the conductors should be so large that the electric heating is almost nil.

24. The insulation on any conductor other than a flexible cord must be throughout either—

(*a*) A dielectric which is impervious to moisture and only needs mechanical protection. ("Dielectric" does not include braiding or taping.) Or

(*b*) A dielectric which must be kept perfectly dry, and therefore needs to be encased in a waterproof sheath, generally of soft metal, such as lead, drawn closely over the dielectric.

[1] See Appendix, p. 513.

25. The radial thickness of vulcanised rubber must be not less than 30 mils plus one-tenth of the diameter of the conductor (see Table, column 3). The radial thickness of dielectrics of Class (*b*) must be not less than that given in the Table, column 4. The dielectric must not soften sufficiently to allow decentralisation at a lower temperature than 170° Fahr.

26. The dielectric of Class (*a*) must be thoroughly damp-proof, and that of Class (*b*) must be enclosed in a sheath of ductile material entirely impervious to moisture, which, if metallic, must be electrically continuous throughout and connected to earth.

27. The dielectric must be such that when a test-piece of the insulated conductor has been immersed in water for twenty-four hours it will, while still immersed, withstand 2,000 volts for ten minutes between the conductor and the water. Prior to immersion the test-piece must have been bent six times (three times in one direction and three times in the opposite direction) round a smooth cylindrical surface not more than twelve times the diameter of the finished cable.

28. The minimum insulation resistance should be that given in Column 12 of the Table for vulcanised rubber, and that in Column 13 for Class (*b*), the test being made at 60° F. after one minute's electrification at 500 volts, and after the test-piece has been immersed in water for twenty-four hours. This resistance must not fall more than 10 per cent. after seven days' immersion.

29. Conductors insulated as in Class (*a*) may be protected by braid or taping, prepared so as to resist moisture. Unless fixed in sight and out of reach of injury, all conductors must, further, be protected by a strong covering; and this, in damp situations, must consist of water-tight, incombustible tubes, which, if of metal, must be electrically continuous throughout and connected to earth. Means must be provided to prevent the accumulation within the tubing of water arising from condensation or other sources. Sharp bends or elbows must be avoided, corners being turned by smooth-bore round bends or suitable boxes.

30. The exposed ends of conductors, with dielectrics of Class (*b*), where they enter the terminals of switches, fuses, and other appliances, must be protected from moisture which might creep along the insulating material within the waterproof sheath.

31. Concentric conductors should in all respects conform to the requirements herein laid down for single conductors; the insulation resistance of the dielectric separating the two con-

ductors must be that given in the Table for single conductors having the same diameter as the inner conductor. The insulation resistance of the dielectric on the outer conductor where insulated, must be that given in the Table for single conductors of the same outside diameter.

32. When the mains are earthed at one point, the outer conductor of a concentric system is the conductor to be connected to the earthed main.

33. In applying the bending test to concentric conductors, the diameter of the cylinder used should be not more than twelve times the diameter of the finished cable.

34. Flexible conductors, *i.e.*, those made up of a number of wires not larger than No. 35 S.W.G., which are then insulated, may only be used for attachment to portable appliances or pendants or for sub-circuits when visible throughout their length and spaced from walls by porcelain insulators. For the wiring of fittings a strand composed of three wires of No. 25 S.W.G. may be used. The insulating material used as the dielectric must be either pure rubber or vulcanised rubber of the best quality. If pure rubber be used, it should be laid on in two layers, care being taken that these break joint. The radial thickness of the dielectric must not be less than 16 mils for pressures up to 125 volts, or 20 mils for pressures from 125 to 250 volts. The covering must be such that a test-piece not less than one yard in length cut from the conductor will withstand a pressure of 1,000 volts alternating at a frequency of from 40 to 100 periods per second applied for ten minutes between the test-piece and a similar test-piece twisted together with it, the pieces being subjected during the test to the vapour arising from a pan of boiling water placed ten minutes before the commencement of the test at a distance not exceeding three feet immediately below it.

CONDUCTORS—JOINTS.

35. Joints in conductors are prohibited except on small wires protected by fuses, viz., 5-ampere fuses on circuits up to 125 volts, and 3-ampere fuses on circuits from 125 to 250 volts. Junction-boxes must be used to connect lengths of larger conductors, and be so constructed that—

- (*a*) the conductors cannot be readily short-circuited;
- (*b*) the insulation between opposite poles will not readily break or chip;
- (*c*) the connections do not heat.

If used in damp places, special precautions must be adopted to exclude moisture.

36. Joints must be mechanically and electrically perfect to prevent heat being generated. All joints must be soldered. Soldering fluids containing acid, or other corrosive substances, must not be used. The insulation of all joints in insulated conductors must be most carefully attended to.

37. In jointing conductors the braiding, tape or lead, must be carefully removed without damage to the dielectric for a sufficient length to insure a thorough union between the dielectric and the material used to insulate the joint. If the insulating material is not waterproof, it must be covered with an impervious sleeve or box, which must make a water-tight joint on each side of the junction. Care must be taken to exclude moisture during the operation.

38. Joints between flexible conductors and permanent wires under flooring or in wood-casing are prohibited.

Joints constitute a source of weakness, and they must, therefore, be accessible, and it is recommended that their positions be indicated by a conspicuous mark.

BURIED CONDUCTORS IN BUILDINGS.

39. Conductors buried in cement or plaster must be provided with protection of sufficient strength to resist a nail.

40. Conductors passing through walls or fire-resisting floors must be provided with additional protection, such as a porcelain or other incombustible tube which must be filled up with some chemically inert incombustible material, so as to prevent the spread of fire through these openings. When the end is outside the building it should be bell-mouthed and turned downwards.

CONDUCTORS—WOOD CASING FOR.

41. Wood casing must not be—

(*a*) buried in plaster or cement, nor exposed to moisture ;
(*b*) used in damp places ;
(*c*) run immediately below water pipes unless efficiently protected from drip.

CONDUCTORS—PRECAUTIONS AT POINTS OF CONNECTION.

42. Where conductors are connected to switches, fuse junction-boxes, or other appliances, the whole of the separ wires forming the stranded or flexible conductor must ma

contact with the terminal so that no loose wire or strand can project. The dielectric must not be bared back further than to allow the conductor to enter the terminals properly, and the ends of the insulation Class (*b*) should be sealed.

43. The braiding, lead, or other covering to the dielectric must be cut back from the end of the insulating material, and waterproofed. In damp places the strands of conductors, Class (*b*), should be soldered to prevent moisture creeping along the copper beneath the insulation.

44. Conductors of larger section than 7/18 must be soldered to proper lugs for connection. Where there is any possibility of strain on the lugs they must be mechanically attached in addition to the soldering.

SWITCHES.

45. Every switch, whether fixed separately or combined with lampholders or fittings, must, except as provided in paragraph 17, be encased, and comply with the following requirements :—

(*a*) Overheating must not take place at the point of contact or elsewhere, when the full current flows continuously.

(*b*) When being switched off it must not be possible to form a permanent arc. Switches should be tested at pressure and current 50 per cent. in excess of that which will be used on the circuits for which they are intended.

(*c*) It must be incapable of remaining in partial contact.

(*d*) The base must be of incombustible non-conducting and moisture-proof material.

(*e*) The cover must be of incombustible material, and must be either non-conducting, or of rigid metal, and clear of all internal mechanism.

(*f*) Where the pressure exceeds 250 volts, covers must be of metal and must be earthed.

(*g*) Handles must be insulated and so arranged that the hand cannot touch live metal.

(*h*) It must not contain a fuse.

FUSES.

46. Every fuse must be encased, except as provided in paragraph 17, and comply with the following requirements :—

(*a*) That no overheating can take place in any part when the full current flows continuously.

(*b*) That it shall effectually interrupt the current when a short-circuit occurs, and also when the current through it exceeds the working rate by 100 per cent., the current flowing under the normal pressure in both cases.

(*c*) The base of the fuse must be of incombustible, non-conducting, and moisture-proof material.

(*d*) The cover must be of incombustible material, and must either be non-conducting or of rigid metal lined with insulating incombustible material. It must be kept clear of all the internal mechanism. When the fuses are of the open type and grouped together, the case of the distribution board will be a sufficient protection provided the distance from cover to fuse exceeds two inches.

(*e*) Fuses must not be placed in wall-sockets, ceiling roses, lampholders, or switch covers.

(*f*) The fusible metal must be of such size that no conductor protected by it can possibly exceed the temperature specified in paragraph 23.

47. Separate single fuses, and not "double-pole" fuses, must be used on circuits on which the pressure exceeds 125 volts.

48. Fuses may be considered too large if they are not warm to the touch on full load, and too small if they hiss when moistened.

49. *Note:* It is recommended that hard metal be used for fuses; and that if soft wire is used, it should be soldered to hard metal contacts.

CONNECTORS: WALL- AND FLOOR-PLUGS, ETC.

50. All connectors should be capable of withstanding a test at a pressure and current 50 per cent. in excess of that for which they are intended. In damp places special water-tight connectors must be used. In cases where the fixed part of the connector is attached to a floor it must be so arranged that no dust or water can accumulate in the cavity, and that all contacts are well below the floor-level, or covered to prevent any possibility of danger from contact with carpets.

51. No connector may contain a fuse.

52. Connectors must be constructed so that they cannot be readily short-circuited. Clearances should be such that an arc cannot be started if the connector is pulled out at the time that the current is flowing. The insulation used between opposite poles should be such that it will not readily break or chip.

53. Flexible cord conductors for portable fittings must end in a connector.

54. Every portable current-consuming device must be independently controlled by a switch on the live side of the connector.

CEILING ROSES.

55. Every ceiling rose must comply with the following requirements :—

(*a*) The base must be of incombustible, non-conducting and moisture-proof material ;

(*b*) The cover must be of incombustible material, and must be either non-conducting or of rigid metal, and clear of all internal mechanism ;

(*c*) Unless it, or its base, form part of the sheathing as in paragraph 12, it must not be attached directly to a plastered surface, but must be mounted on a prepared block ;

(*d*) Its terminals must be relieved of the direct pull of the attached conductor and fitting, and be so arranged that no short circuit can take place ;

(*e*) It must not contain a fuse.

SWITCH AND DISTRIBUTION BOARDS.

56. Main and distribution switch- and fuse-boards must be made of incombustible insulating material insulated, where hygroscopic, by bushes from the supporting framework, and fixed in a dry situation, and be so placed that a fire thereon cannot spread to combustible material.

57. Live metal must be fixed at such a distance from all metal not at the same potential, or be so separated by insulating partitions, that an arc cannot be formed between the metal surfaces.

58. Connections at the back of boards must be made accessible, but, unless protected from acid fumes, must not project into battery rooms. Circuits should be labelled for identification.

59. The cases of instruments, if metallic, must be insulated from the circuits, or, if connected to one pole, they should be protected from the possibility of contact with the other.

60. Every voltmeter with its connecting wires should be protected by a fuse on each pole.

FITTINGS FOR SUPPORTING LAMPS.

61. Wherever brackets, electroliers, or standards, require to have the conductors threaded through tubes or channels

formed in the metal work, these must be of ample size and have no sharp angles or projecting edges, which would be liable to damage the insulating material.

62. Where possible, the conductors should be carried without joints through the fittings to the lamps; but where connections at the fitting are unavoidable, special care must be taken to make the joints equal in conductivity and insulation to the rest of the work.

63. Combined gas and electric fittings must not be used.

64. When disused gas-fittings are adapted for electric light, they must be entirely disconnected from the gas-pipes.

LAMPHOLDERS.

65. Lampholders must—

(*a*) be entirely incombustible;
(*b*) be insulated from any continuously earthed conduit or sheath not forming part of the circuit;
(*c*) be specially designed if for currents above $1\frac{1}{2}$ amperes;
(*d*) not be hung from flexible cord conductors where exposed to the weather, but be rigidly supported.

66. Switch lampholders should be controlled in groups of ten, or fewer, by a separate fixed wall-switch.

ARC LAMPS.

67. Arc lamps must—

(*a*) be guarded by lanterns or globes, which must be arranged to intercept falling particles of carbon;
(*b*) be insulated from their support;
(*c*) be fixed so that their cases cannot come into contact with any metallic object;
(*d*) have their leading-in wires protected from rain;
(*e*) be controlled by linked switches and protected by fuses (see "General Arrangements");
(*f*) not be used in places where inflammable vapours or explosive mixtures of dust or gas are liable to be present.

INCANDESCENT LAMPS.

68. Incandescent lamps and their holders—

(*a*) must not be placed in close proximity to inflammable materials; shades made of such materials

must be kept free from contact with the lamps by suitable guards; celluloid and other highly inflammable material must not be used for shades;

(*b*) if placed in positions where they are exposed to inflammable vapour or gas, should be enclosed in air-tight fittings of thick glass and have no flexible cord connections.

69. Incandescent lamps of the Nernst type must comply with the regulations of paragraphs 67 (*a*), (*b*), (*c*), (*d*), (*f*) and 68 (*a*).

DYNAMOS AND MOTORS.

70. Any dynamo or motor rated at more than one-third of a horse-power must—

(*a*) be protected from damp, dust, and mechanical injury;

(*b*) be so placed that no unprotected woodwork or combustible material is within a distance of twelve inches from it measured horizontally, or within four feet measured vertically above it, unless it is of an enclosed type;

(*c*) if supplied at 250 volts or upwards, have its frame efficiently connected to earth;

(*d*) if employed in positions exposed to highly inflammable dust or flyings, or where highly inflammable materials are manipulated or stored, be of the enclosed type, without belting or gearing penetrating the casing, with ventilating openings, if any, only in the vertical portions of their casings, protected by two thicknesses of fine-mesh wire gauze set at least a quarter of an inch apart and substantially attached to the casing;

(*e*) be controlled by linked switches and protected by fuses or circuit-breakers on both conductors;

(*f*) if a motor, have, in addition to the above, starting gear consisting of a regulating switch and series resistance, the regulating switch being fitted with a magnetic release that will automatically open the circuit should the current be interrupted.

Note.—It is recommended that all shunt circuits of motors be arranged so that the field is excited before the armature is connected, these circuits to be disconnected through a non-inductive resistance or carbon break after the armature circuit is broken.

RESISTANCES.

71. Resistances, whether used in connection with arc-lamps, dynamos, or motors, or for any other purpose, must be—

(*a*) carried on frames or supports and enclosed in cases, the frames, supports, and cases to be of incombustible material efficiently insulated from the resistances ;

(*b*) amply ventilated by means of apertures protected by fine-mesh wire gauze where there is danger of inflammable material entering them ;

(*c*) so proportioned that they cannot rise in temperature more than 240° F., nor the cases containing them more than 130° F., above the temperature of the surrounding air ;

(*d*) so fixed that no unprotected inflammable material is within six inches of the cases containing them, or within twenty-four inches measured vertically above them.

CHOKING COILS.

72. Choking coils must comply with the rules for Resistances (71, *a*, *b*, and *d*, and 76).

ACCUMULATORS AND OTHER BATTERIES.

73. The room in which accumulators or primary batteries are placed must be well ventilated.

74. Accumulators and batteries must be well insulated from earth, and protected by fuses at all points of connection between the circuit and the regulating cells, unless special precautions are taken to keep the conductors permanently apart by incombustible and non-conducting material.

TRANSFORMERS.

75. If high-pressure transformers are brought into a building they must—

(*a*) together with their switches and fuse-boxes, be contained in fire- and water-proof structures, and be accessible only to authorised persons ;

(*b*) be so protected by suitable apparatus that a leak between the primary and secondary coils shall cut the transformer out of circuit ;

(*c*) not under normal full-load exceed a temperature of *170° F.*

76. Low-pressure alternating transformers or choking coils must conform with paragraphs 71 (*a*), (*b*), and (*d*), and their temperature must not exceed 170° F.

ELECTRIC COOKING APPLIANCES, RADIATORS AND HEATERS.

77. These appliances must be—

(*a*) so constructed and mounted that heat cannot be conveyed to their supports and connections, precautions being taken with regard to their surroundings as in the case of non-electrical heating appliances ;

(*b*) protected by a fuse and switch in both conductors, subject to Rule 5, connectors being so arranged that the live end of the coupling is not exposed to accidental short-circuiting or injury.

TESTING.

78. The insulation resistance to earth of the whole or any part of the wiring must, if tested previously to the erection of fittings and electroliers, be measured with a pressure not less than twice the intended working pressure, and must not be less in megohms than 30 divided by the number of "points" under test. For this purpose the "points" are to be counted as the number of pairs of terminal wires from which it is proposed to take the current, either directly, or by flexibles, to lamps or other appliances.

79. Current must not be switched on until the following test has been applied to finished work :—

The whole of the lamps having been connected to the conductors and all switches and fuses being on, a pressure equal to twice the working pressure must be applied and the insulation resistance of the whole or any part of the installation must not be less in megohms than 25 divided by the number of 30-watt lamps. When all lamps and appliances have been removed from the circuit, the insulation resistance between conductors must not be less than 25 megohms divided by the number of 30-watt lamps. For the purpose of this test, every arc light shall be considered as equivalent to 15 lamps, and every motor or heater shall be rated at one lamp per ampere, provided that no motor, heater, or other appliance may be connected to the supply of electrical energy unless the insulation of the parts carrying the

current measured, as above, is greater than 500,000 ohms from the frame or case.

80. The value of systematically inspecting and testing apparatus and circuits cannot be too strongly urged as a precaution against fire. Records should be kept of all tests, so that any gradual deterioration of the system may be detected. Cleanliness of all parts of the apparatus and fittings is essential. In testing, the negative pole should be connected with the conductor under test.

81. No repairs or alterations may be made when the pressure is "on."

EXPLANATION OF TABLE.

82. *Columns 1 and 16* give the size of the conductors in common use. Cables are shown thus :—19/16, viz., 19 wires of number 16 standard wire gauge, or 19/·082", meaning 19 wires each of which is ·082 inch in diameter.

83. *Column 2* gives the section of the conductor in square inches.

84. *Column 3* gives the minimum thickness of dielectric as defined in paragraph 25 on vulcanised rubber cables.

85. *Column 4* gives the minimum thickness of dielectric on fibre-covered cables which require to be lead-covered, viz., cables of Class B. Special cables, such as twin or 3-core cables, are not included in this column.

86. *Column 5* gives the safe radial thickness of lead in decimals of an inch for cables of Class B. This column does not apply to vulcanised rubber cables which may be lead-covered.

87. *Column 6* gives the maximum current for wires insulated with vulcanised india-rubber laid in position within the mechanical protections allowed in the Rules, when the external temperature is higher than 100° F. The current for any conductor under these conditions may be calculated from the formula :—

$$\text{Log } C = 0{\cdot}775 \log A + 0{\cdot}301,$$
$$C = 2\,A^{0{\cdot}775}$$

(where C = current in amperes, A = area in 1,000ths of a square inch).

The maximum rise in temperature will be about 10 degrees Fahrenheit on large sizes.

88. *Column 7* gives the maximum current allowable in any situation for conductors insulated with vulcanised rubber when

laid in positions within the mechanical protections allowed in the Rules when the external temperature is normal. The maximum current for any conductor may therefore be calculated from the formula—

$$\text{Log } C = 0{\cdot}82 \log A + 0{\cdot}415,$$
$$\text{or } C = 2{\cdot}6\ A^{0{\cdot}82}$$

89. *Column 8* gives the total length in yards of lead and return of each size of conductor, causing a drop of 1 volt when transmitting the current shown in Column 6.

90. *Column 9* gives the current density in amperes per square inch corresponding to Column 8.

91. *Column 10* gives the maximum allowable current with lead-covered cables, allowing a rise of about 20° F. on large sizes.

92. *Column 11* gives the total length in yards of the conductor (lead and return) for one volt drop when the current in each conductor is that given in Column 10.

93. *Column 12* gives the minimum insulation resistance with vulcanised rubber in mile-megohms. These insulation resistances correspond approximately with those of "300 megohm grade" cables having a specific insulation of 1·4.

94. *Column 13* gives the minimum insulation resistance which is advisable in practice for fibre-covered cables lead-covered. The insulation resistance between the members of twin-conductors should be not lower than the corresponding insulation resistances in the Table.

95. *Column 14* gives the resistance in Board of Trade ohms of the conductor per 1,000 yards.

96. *Column 15* gives the weight of copper conductors of the gauge given in lbs. per 1,000 yards.

DEFINITIONS OF CERTAIN TERMS USED IN ABOVE RULES.

97. *Bunching of Conductors.*—Conductors are said to be bunched when more than one is contained within a single duct or groove.

98. *Dielectric.*—A dielectric is any material which by its nature or the method of its application to a conductor permanently offers high resistance to the passage of current and of disruptive discharge through itself.

99. *Earthed Conductor.*—A conductor is said to be earthed when it is metallically connected at one or more points to the general mass of the earth.

100. *Linked Switches.*—Linked switches are single-pole switches fixed on conductors of different polarity linked together mechanically so as to operate simultaneously.

101. *Neutral Conductor.*—The neutral conductor of a three-wire system is the conductor which is at a potential intermediate between the potentials of the outer conductors, and is common to all consuming devices.

102. *Outer Conductor.*—The outer conductors of a three-wire system are those between which there is the greatest difference of potential.

Note.—This specialised use of the word "outer" must not be confused with the non-technical use of the word when applied to the conductor of a concentric main which physically surrounds the other conductor or conductors of such main.

103. *Single-pole Switches.*—Single-pole switches are switches interrupting one conductor only of a circuit.

104. *Three-wire System.*—A three-wire system is one in which three conductors are maintained at different potentials, the conductor at a potential intermediate between the highest and lowest being common to all lamps or other consuming devices supplied from the system.

105. *Uninsulated Conductor.*—A conductor is said to be uninsulated when, although not metallically connected to earth, no provision is made by the interposition of a dielectric or otherwise for its insulation from earth.

APPENDIX.

The data for the resistances and weights of copper conductors are based on Matthiessen's standard as defined by the Committee on Copper Conductors in 1899, as follows :—

"Copper weighs 555 lbs. per cubic foot at 60° F. Its specific gravity = 8·912.

"Weight per mile in lbs. = 20, 350 × area in square inches.

"Weight per yard in lbs. = 11, 5625 × area in square inches.

"The temperature coefficient = 0·00238 per degree Fahr., or 0·00428 per degree Cent. = 0·07664 between 32° F. and 60° F.

"A lay of twenty times the pitch diameter is adopted as a standard, and the resistance in parallel of the wires is taken as the resistance of the cable.

"The resistance of ***annealed*** high conductivity commercia copper at 60° F. is :—

"Resistance per cubic inch = 0·00000066788 standard ohm-

"Resistance per cubic cm. = 0·00000169639 ,, ,,

"Resistance of 100 inches weighing 100 grains = 0·150158 ,, ,,

"The resistance of ***hard-drawn*** high conductivity commerci copper is :—

"Resistance per cubic inch = 0·000000681327 standard ohm

"Resistance per cubic cm. = 0·00000173054 ,, ,,

"Resistance of 100 inches weighing 100 grains = 0·153181 ,, ,,

The above formulæ give the standards, but a variation o 2 per cent. in resistance or weight may be allowed for losses in manufacture.

INDEX TO WIRING RULES.

(*Note: the References given denote the numbers of paragraphs, not of pages.*)

…UCTORS INSULATED AND LAID IN CASING OR TUBING.

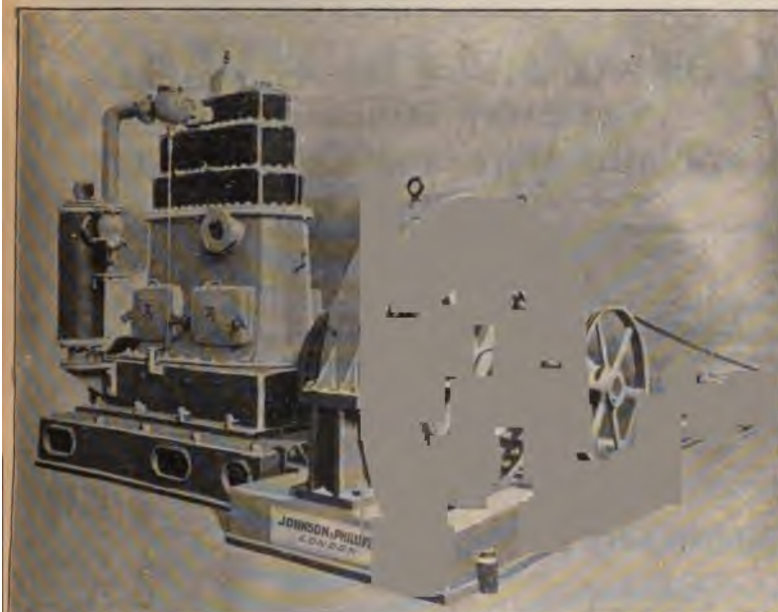
11.	12.	13.	14.	15.	16.
…olt drop. …tal length in …rds of lead …d return for …-volt drop …ol. 7 and Col. 10).	*Insulation.* Minimum insulation resistance for rubber.	*Insulation.* Minimum insulation resistance with thickness of dielectric of Column 4 for Class B.	Conductor resistance in B.O.T. standard ohms per 1,000 yards.	Weight of copper in lbs. per 1,000 yards.	Number of wires and gauge in S.W.G. or inches.
	Mile-Megohms.	Mile-Megohms.			
18	1,200	300	13·28	20·93	1/18
18	1,200	270	13·18	21·62	3/22
19	1,200	270	9·762	28·48	1/17
19	1,200	250	7·972	35·75	3/20
20	1,200	250	7·478	37·2	1/16
21	800	230	5·904	47·09	1/15
21	800	220	5·636	50·36	7/22
21	800	220	4·784	58·13	1/14
22	800	200	4·482	63·52	3/18
23	600	180	3·41	83·3	7/20
25	600	160	1·918	148	7/18
27	600	140	1·257	226·3	19/20
28	600	140	1·080	263·1	7/16
30	450	120	0·7074	402·2	19/18
31	450	120	0·6903	411·1	7/14
32	450	120	·490	580	7/·095″
32	450	120	·488	591	19/·058″
33	450	110	·3981	714·8	19/16
36	300	100	·2547	1,117	19/14
36	300	100	·244	1,182	19/·082″
38	300	90	·2045	1,393	37/16
38	300	90	·194	1,488	19/·092″
39	300	90	·160	1,781	19/·101″
39	300	90	·163	1,776	37/·072″
41	300	90	·1507	1,888	19/12
42	300	90	·1309	2,176	37/14
40	300	90	·125	2,303	37/·082″
42	300	80	·09795	2,907	61/15
40	300	80	·0997	2,900	37/·092″
…3	300	80	·0827	3,494	37/·101″
…5	300	80	·07937	3,589	61/14
[illegible]	300	80	·07744	3,678	37/12
[illegible]	300	80	·0697	4,145	37/·110″
[illegible]	300	80	·0606	4,772	37/·118″
[illegible]	300	80	·0605	4,781	61/·092″
[illegible]	300	80	·0502	5,762	61/·101″
[illegible]	300	80	·04697	6,065	61/12
[illegible]	300	80	·0423	6,836	61/·110″
[illegible]	300	80	·0405	7,134	91/·092″
[illegible]	300	70	·0357	8,094	91/·098″
[illegible]	300	70	·0336	8,597	91/·101″
[illegible]	300	70	·0317	9,115	91/·104″
[illegible]	300	70	·0283	10,200	91/·110″
[illegible]	300	70	·02530	11,256	91/11
[illegible]	300	70	·0246	11,740	91/·118″
[illegible]	300	70	·0239	11,910	127/·101″

…are …e absolutely correct figures which would be found by calculation …xpen…oived in supplying a multiplicity of odd sizes of wire.

BUYERS' GUIDE.

Being a Classified Index of Names, Addresses, and Specialities of Firms Advertising in this Journal.

(Where no folio is given Advertisement does not appear in this Issue.)



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